Med
B

Third Quinquennial Series.—No. IV.

STORAGE

THE BRITISH AND FOREIGN MEDICAL REVIEW,

OR

Quarterly Journal

OF

PRACTICAL MEDICINE AND SURGERY.

EDITED BY

JOHN FORBES, M. D., F. R. S., F. G. S.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS IN LONDON;

Honorary Member of the Cambridge Philosophical Society, of the Academy of Sciences of Madrid, of the American Philosophical Society; of the Imperial and Royal Society of Physicians of Vienna, of the Royal Society of Göttingen, of the Royal Medical Society of Copenhagen, of the Medico-Chirurgical Society of Amsterdam, of the Medico-Chirurgical Society of Turin, &c., &c.; Physician in Ordinary to Her Majesty's Household; Physician Extraordinary to His Royal Highness Prince Albert; Physician in Ordinary to His Royal Highness the Duke of Cambridge; and Consulting Physician to the Hospital for Consumption and Diseases of the Chest.

VOL. XXII.

No. XLIV.

OCTOBER, 1846.

LIBRARY
SEP 21 1943
UNIVERSITY OF TORONTO

AMERICAN EDITION.—No. II.

415824
21.9.43

PHILADELPHIA:
G. B. ZIEBER AND CO.

C. Sherman, Printer, 19 St. James Street.

PRICE THREE DOLLARS A-YEAR, IN ADVANCE.

HIGHLY IMPORTANT

TO THE MEDICAL PROFESSION

OF THE UNITED STATES.

Republication of the British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery, edited by John Forbes, M. D., F. R. S., F. G. S., &c., &c.,—*one of the most important and widely circulated periodicals of the present day.*

The acknowledged value of this work, and the reception which it every where meets with by the medical profession in its rapidly extending circulation, has rendered a thorough republication of it in this country, at a moderate price, very desirable to a great part of the medical world.

In announcing their design to republish it as a fac-simile of the European work, the American publishers have met with such a degree of encouragement as has exceeded their most sanguine expectations, and enabled them to proceed immediately with the work—a sufficient number of subscribers' names having already been obtained to insure its prompt appearance.

Digitized by the Internet Archive
in 2010 with funding from
University of Toronto

The republication commences with the number for July, 1846, and will be continued in the succeeding numbers on their arrival, with the least possible delay. The size of the pages, quantity of matter on each page, type, quality of paper, and all the minor points in the republished work, will be made to conform with precision to the original, in order that the uniformity of the original publication shall be strictly carried out in the American edition. This will enable those who have heretofore subscribed to the English work, and who may wish to change to the American, AT LITTLE MORE THAN HALF THE COST, to do so, without any interference with the size or appearance of the periodical, as the two may be bound together, without any inconvenience whatever.

The character of the work thus to be republished, requires no comment from the American publishers, who, in thus offering their edition to the medical public, have found it well known, and its importance fully appreciated. They may be permitted to say, too, that in their endeavours to render it more extensively known, and available to all classes of readers, their reception has already been very flattering to their project.

The terms of subscription to the American edition will be Three Dollars per annum, payable in advance.

Address, G. B. ZIEBER & CO., Publishers,
Philadelphia.

http://www.archive.org/details/britishforeignme22philuoft

CONTENTS OF NO. XLIV.

OF THE

British and Foreign Medical Review.

OCTOBER, 1846.

PART FIRST.—Analytical and Critical Reviews.

PAGE

ART. I.—The Nature and Treatment of Cancer. By W. H. WALSHE, M. D., Professor of Pathological Anatomy in University College, Physician to University College Hospital, and to the Hospital for Consumption and Diseases of the Chest - - - - - - - - 293

Cancer of the Digestive Organs and Appendages - - - - ib.
Cancer of the Respiratory Organs and Appendages - - - - 300
Cancer of the Kidney - - - - - - - - 303
Cancer of the Bladder - - - - - - - - 305
Cancer of the Genital Organs - - - - - - 306
Cancer of the Organs of Innervation and Appendages - - - 309

ART. II.—Die Gallige Dyscrasie (Icterus) mit acuter gelber Atrophe der Leber. Von Dr. P. J. HORACZEK, practischen Arze in Wien, &c. - - - 311

On Bilious Dyscrasy (Icterus) with Acute Yellow Atrophy of the Liver. By Dr. P. J. HORACZEK.

ART. III.—Transactions of the Provincial Medical and Surgical Association. New Series. Vol. II. - - - - - - - - 319

Mr. Crosse's History of the Cure of a Tumour - - - - ib.
Dr. Favell on Grinders' Asthma - - - - - - - 320

ART. IV.—1. Pathologische Anatomie des menslichen Körpers. Von JULIUS VOGEL. Erste Abtheilung. (Allgemeiner Thiel.) - - - 323

The Pathological Anatomy of the Human Body. By JULIUS VOGEL.

PAGE

2. Julii Vogel Icones Histologiæ Pathologicæ. Tabulæ Histologiam Pathologicam illustrantes. Viginti sex Tabulæ, continentes ccxci Figuras, quarum cclxx ad naturam delineatæ sunt - - - - - - 323

Erläuterungstafeln zur pathologischen Histologie mit vorzüglicher Rücksicht auf sein Handbuch der pathologischen Anatomie, herausgegeben von Dr. Julius Vogel, ausserordentl. Professor der Medizin in Göttingen. Sechs und zwanzig Tafeln mit 291 Figuren, wovon 270 nach der Natur gezeichnet sind.

Illustrations of Pathological Histology, illustrative of his Manual of Pathological Anatomy. By Dr. Julius Vogel, Extraordinary Professor of Medicine at Göttingen.

Art. V.—Geschichte der Findlinge in Oesterreich mit besonderer Rücksicht auf ihre Verhältnisse in Illyrien. Von Dr. Raimund Melzer, K.k. Director der Staats-und Local-Wohlthätigkeits-Anstalten zu Laibach - 338

History of Foundlings in Austria, with especial Reference to their Condition in Illyria. By Dr. Raimund Melzer, Imperial Director of the State and Local Charitable Institutions of Laybach.

Art. VI.—Observations and Essays on the Statistics of Insanity; including an inquiry into the Causes influencing the Results of Treatment in Establishments for the Insane: to which are added the Statistics of the Retreat, near York. By John Thurnam, Licentiate of the Royal College of Physicians of London, Resident Medical Superintendent of the Retreat, near York - - - - - - - - - - 349

On the Statistics of Insanity in general - - - - - ib.

Essays on the Liability to Insanity - - - - - - 360

Historical Sketch and Statistics of the Retreat - - - - 363

Art. VII.—1. Practical Surgery. By Robert Liston. Fourth edition - - 366

2. Lectures on the Operations of Surgery, and on Diseases and Accidents requiring Operations. By Robert Liston, Esq., f.r.s.; with numerous Additions, by Thomas D. Mütter, m.d., Professor of Surgery in Jefferson College, Philadelphia, &c. - - - - - - - 379

Art. VIII.—Die Acute Entzündung der Serösen Häute der Gehirns und Rückenmarks. Nach eigenen Beobachtungen am Krankenbett geschrieben. Von Dr. Joseph Neisser, Prakt. Arzt zu Berlin - - - - 384

Acute Inflammation of the Serous Membranes of the Brain and Spinal Cord; described from Personal Observation at the Bedside. By Dr. Joseph Neisser, Physician at Berlin.

Art. IX.—On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart. By George Burrows, m.d., late Fellow of Caius College, Cambridge, Fellow of the Royal College of Physicians, London, Physician and Lecturer on the Principles and Practice of Medicine at St. Bartholomew's Hospital - 408

Art. X.—1. Examen Clinique de l'Hydrothérapie. Par H. E. Schedel, Docteur en Médecine - - - - - - - - - 428

PAGE

2. The Dangers of the Water Cure, and its Efficacy Examined. By JAMES WILSON, M.D., and JAMES M. GULLY, M.D. - - - - - 428

3. The Cold Water Cure: its Use and Misuse examined. By HERBERT MAYO, M.D., F.R.S., formerly Surgeon of Middlesex Hospital - - ib.

4. A Medical Visit to Graeffenberg. By Sir CHARLES SCUDAMORE, M.D., F.R.S. - - - - - - - - - - ib.

5. Hydropathy. By EDWARD JOHNSON, M.D. - - - - - - ib.

6. Graeffenberg; or, a True Report of the Water Cure, with an Account of its Antiquity. By ROBERT HAY GRAHAM, M.D. - - - - ib.

7. Life at the Water Cure, or a Month at Malvern. By R. J. LANE - - ib.

8. Confessions of a Water Patient. By Sir E. B. LYTTON, Bart. - - ib.

ART. XI.—Redogörelse Sjukvården à Kongl. Seraphimer Lazarettets Afdelning för Invertes Sjuke, under Loppet af Ar 1842 - - - - 459

Medical Report of the Internal Diseases treated in the Royal Seraphim Hospital in Stockholm during the year 1842. By Dr. MAGNUS HUSS.

ART. XII.—Mesmerism in India, and its practical Application in Surgery and Medicine. By JAMES ESDAILE, M.D. - - - - - 475

ART. XIII.—The Brain and its Physiology; a Critical Disquisition on the Methods of determining the Relations subsisting between the Structure and Functions of the Encephalon. By DANIEL NOBLE, M.R.C.S. - - - 488

ART. XIV.—On the Injurious Effects of Mineral Poisons in the Practice of Medicine, &c. &c. By HORATIO PRATER, M.D., PH.D., &c. - - - 545

ART. XV.—A Practical Treatise on the Diseases of Children. By JAMES MILLMAN COLEY, M.D., Member of the Royal College of Physicians in London, &c. - 548

PART SECOND.—Bibliographical Notices.

ART. I.—Notes and Recollections of a Professional Life. By the late WILLIAM FERGUSSON, M.D., Inspector-General of Military Hospitals. Edited by his Son, JAMES FERGUSSON - - - - - - - 551

ART. II.—The Microscopic Anatomy of the Human Body in Health and Disease, illustrated with numerous Drawings in Colour. By ARTHUR HILL HASSALL, F.L.S., M.R.C.S., &c. Parts I. and II. - - - - - 552

ART. III.—Elements of Physics. By C. F. PESCHEL, Principal of the Royal Military College at Dresden, &c. Translated from the German, with Notes, by E. WEST - - - - - - - - 553

PAGE

ART. IV.—The Vegetable Kingdom; or the Structure, Classification, and Uses of Plants, illustrated upon the Natural System. By JOHN LINDLEY, F.R.S., F L.S., Professor of Botany in the University of London, and in the Royal Institution of Great Britian - - - - - - - - 554

ART. V.—The Anatomy of the Arteries of the Human Body, and its Applications to Pathology and Operative Surgery; with a series of Lithographic Drawings the Size of Nature. By RICHARD QUAIN, F.R.S. The Drawings by JOSEPH MACLISE, Esq., Surgeon - - - - - - 556

PART THIRD.—Natural History and Treatment of Diseases.

I.—On Excessive Trust in Nature in the Cure of Diseases. By J. A. SYMONDS, M.D., Consulting Physician to the General Hospital, Bristol - - - 557

II.—On the Present State of Homœopathy in Germany. By Dr. A. MÜHRY, of Hanover - - - - - - - - - 565

III.—Report on the Homœopathic Treatment of Acute Diseases in Dr. Fleischmann's Hospital, during the months of May, June, and July, 1846. By GEORGE W. BALFOUR, M.D., Edin. - - - - - - 567

Typhus Fever - - - - - - - - 573
Febris Gastrica - - - - - - - - 576
Intermittent Fever - - - - - - - - ib.
Rheumatism - - - - - - - - - 577
Diarrhea - - - - - - - - - 579
Dysentery - - - - - - - - - ib.
Cholera - - - - - - - - - 580
Colica - - - - - - - - - ib.
Scarlatina - - - - - - - - - 581
Pneumonia - - - - - - - - - ib.
Other Diseases - - - - - - - - 591
Remarks by the Editor - - - - - - - 592

Books received for Review - - - - - - - 594

INDEX, TITLE, &c.

THE

BRITISH AND FOREIGN

MEDICAL REVIEW,

FOR OCTOBER, 1846.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

The Nature and Treatment of Cancer. By W. H. WALSHE, M. D., Professor of Pathological Anatomy in University College, Physician to University College Hospital, and to the Hospital for Consumption and Diseases of the Chest.—*London*, 1845. 8vo, pp. 590.

WE proceed with our analysis of the second division of this valuable work, in which the disease is described as it affects various organs, tissues, and parts.

CANCER OF THE DIGESTIVE ORGANS AND APPENDAGES.

1. *Lips.* Labial cancer, without being very common, is of tolerable frequency. There is some variety in its mode of origin and progress. It may commence by infiltration of scirrhus in a limited portion of the lip, underneath the skin, at its junction with the mucous membrane. After a time one of these tissues gives way, leaving a fissure which discharges a thin acrid fluid that dries into a scab. When this falls, or is torn off, it is reproduced, until, as the ulceration advances, no more regular scabs are produced, but fungous granulations spring up from the scirrhous basis. In some cases, the first symptom is said to be a fissure, which resists all treatment. It has been known to commence as a cutaneous warty excrescence, or, as described by Dr. Warren, as a pustule; and it is most certain that venereal ulcerations of this part sometimes become cancerous, though the frequency of such an event has been much exaggerated. But, however commencing, as the disease advances, the skin, mucous membranes, and labial glands form a prominent mass, which, combined with the fungous productions from the sore, produce an abundant slaver that constantly escapes over the lip, and irritates or excoriates the neighbouring skin. The ulceration may spread over the entire cheek, and even part of the external ear. It may affect the maxilla, and has even been known to reach the sternum. The salivary and lymphatic glands become enlarged

and painful, and, especially the latter, may partake of the diseased action. General contamination of the system is comparatively rare. Scirrhus is the usual form of cancer in this part; encephaloid has rarely, if ever, been observed. It is excessively rare in females; and is almost invariably seated in the lower lip. The only affection with which it is likely to be confounded is *venereal ulceration with indurated base;* in many cases the history is the only diagnostic guide.

When the ulceration is superficial, it may sometimes be successfully treated by caustics, the acid nitrate of mercury and the chloride of zinc being the best. It is generally supposed that excision is more successful here than in other parts; but there has been much exaggeration on this point.

2. *Gums.* The term *epulis* is applied to all solid growths from the gingival membrane of either jaw. Cancerous epulis is generally of the scirrhous species. It is more common in the lower than the upper jaw, and usually appears at the reflexion of the membrane on the alveolus, towards the last incisor or bicuspid teeth. It commences by infiltration; but a tuberculated nodule, sometimes with very deep anfractuosities, soon results. It generally varies in size between that of a pea and a nut, but has been said to attain the bulk of a walnut. Ulceration eventually takes place, the sore fungates and sometimes bleeds, and the discharge is peculiarly fetid. The disease destroys the soft parts, and may, but rarely, affect the bone. It is most common in advanced age and in males. There is usually lancinating pain. It is distinguished from the flabby, fungous granulations which sometimes form in the gums, near carious teeth, by its density, its tuberculated shape, and pale colour; and from erectile epulis, by the pulsating characters of the latter. From simple epulis it can scarcely be known, excepting from the result of operation.

3. *Tongue.* Lingual cancer is tolerably frequent. Out of 8289 fatal cases of cancer, reported in the Paris registers, in 36 the main disease was seated in the tongue. Scirrhus is the usual form; but the fungous excrescences sometimes exhibit the encephaloid character. It may commence as a small, somewhat knotty, and irregular tumour, which is generally seated in the anterior part of the organ, midway between the raphe and the edge, and rarely extends beyond the middle line; or the first appearance is a small excrescence, which in some instances is said to become pedunculated. In a few rare cases simple ulcers become cancerous; and still more rarely the morbid matter is deposited in erectile tumours. In either case the surface ulcerates, and the glands become affected in the usual way. Distant organs are not commonly implicated. Acute pain is not a constant symptom; but there is generally an aching sensation, with occasional sharp pangs, darting towards the ear. One of the most distressing concomitants is the necessity for incessant sputation. There are, of course, pain and difficulty in speaking, mastication, and deglutition. These increase with the progress of the disease, until the performance of these functions becomes impossible. The cachexia is frequently intensely marked, and the sufferer is commonly cut off by its progress. According to Mr. Travers, strong and healthy males, æt. 40 and upwards, are the most usual subjects; but the disease is not unfrequently observed in females, and much younger persons.

Lingual cancer may be confounded with *simple induration* of one side of the base of the tongue. The situation and absence of constitutional symptoms will distinguish between the two. Mr. Travers describes a *globular tumour* seated deeply in the organ, which is recognized by its uniform and unyielding surface, and by its disappearance under the use of alkalies and tonics. *Simple ulceration*, produced by mechanical irritation, is usually known by its rapid cure after removal of the cause. *Syphilitic ulcers* are less easily distinguished; they are generally larger, and have less marked and less circumscribed surrounding hardness. The discharge is also less abundant, and they are deficient in the firm everted edge and sprouting fungi. In many cases the history is our only guide. The *fissured* or *dyspeptic ulcer*, which often originates in psoriasis, may bear a close resemblance to cancer, but it has not the hard basis, is often in the middle line, and the rest of the tongue is chapped and irregular. Dr. Warren describes an *enlargement of the mucous glands*, occurring on the side of the tongue, with a red fungous appearance, but differing from cancer in being sensitive, not painful, and unattended by real ulceration or thickening of the organ. *Hypertrophy of the mucous membraue* sometimes gives rise to irregular fissured elevations on the surface. *Hypertrophy of the tongue generally*, a most rare malady, can scarcely be confounded with cancer. *Erectile tumours* are known by their pulsation; but it must be remembered that they may become the seat of cancerous deposits.

In regard to treatment little can be said. Ablation may be practised to avert impending death; but as a curative measure there is no evidence in its favour.

We pass over the description of cancer of the hard and soft palate, the tonsils, and salivary glands, but must say a few words on *pharyngeal carcinoma*. This is rare. Four out of the 8298 cases in the Paris registers were of this kind. The infiltrated form of scirrhus is the most usual species. The disease first presents itself as a hard, imperfectly circumscribed mass, situated in the submucous tissue, and not painful on pressure. Eventually it ulcerates, and passes through the ordinary stages. The symptoms are dysphagia, slight at first, but gradually increasing until deglutition becomes impossible; painful regurgitation of the discharges, and, if the larynx become involved, dyspnea. Hemorrhage is sometimes the first symptom. The patient commonly dies of inanition; and the treatment can be merely palliative. The diagnosis is not always easy. *Simple induration*, with thickening of the submucous tissue, will occasion the same dysphagia, and can only be distinguished by the result of treatment and by the progress of the disease. *Sacculation of the walls of the pharynx*, which also produces some of the symptoms, may be known by the facility with which instruments can generally be passed. *Venereal ulcerations* will be recognised by the history; *polypi* by their form, which is never assumed by cancer in this place; and *enlargement of the cervical vertebræ* by manual examination.

Œsophagus. The registers above noticed give 13 deaths from cancer of this organ. It is most frequently seated in the upper end of the canal behind the larynx, most rarely at its middle part, but it may occupy the whole extent of the tube. Infiltrated scirrhus deposited in a stratiform manner is the usual condition, and it generally commences in the submucous tissue. Distinct scirrhous tumours are comparatively very uncommon; and tuberiform encephaloid still more so. Distant organs are rarely implicated. The *nature* of the symptoms is the same as when the disease is situated in the pharynx, but as the stricture is lower down, the first stages of deglutition are performed naturally. Subsequently, if the upper end is affected, the greater proportion of each mouthful of food at once regurgitates; if a lower part, three or four mouthfuls pass easily, and the accumulated matter is then gently expelled by the action of the muscular fibres. These occurrences are at fisrt only occasional, but eventually the dysphagia becomes permanent. It is necessary, however, to bear in mind that the disease may be intermittent throughout. The œsophagus sometimes forms a large pouch above the stricture, in which the ingesta accumulate; and here, according to Mr. Purton, they undergo a species of chymification. The progress of the disease is accompanied with pain, and obstinate hiccup is not uncommon. Spasmodic dysphagia is a frequent complication, and when some of the food passes into the stomach, it is often rejected by vomiting. The cachexia is commonly marked, and the patient dies of inanition. There is also danger of severe hemorrhage. Perforation may also take place, with effusion into the mediastinum, larynx, trachea, bronchi, pleura, or pulmonary tissue. The diseases stimulating carcinoma of this tube are *spasmodic stricture* and *spasm of the diaphragm*, to be recognized by the history, and the result of treatment; *paralysis* of the muscular coat, known by the easy passage of instruments, and the fact that large masses are more easily swallowed than small ones; *dilatation* and *sacculation* of the tube, also to be diagnosed by the probang; *hypertrophy* and *induration* caused by inflammation, this is generally the result of the ingestion of poisons, or the lodgment of some mechanical irritant, and the history is therefore our guide. Enlarged glands, abscesses, aneurisms, &c., may also cause great obstruction, but a careful examination will prevent error. The treatment can be merely palliative; but as some organic strictures have been cured by persevering dilatation, it will be well to give this plan a trial, and probably Dr. James Arnott's system of fluid pressure would be the best method. Leeching the throat often gives great relief, and benefit is sometimes derived from the application of the lunar caustic, to alter the condition of the sore, when ulceration has taken place. The œsophagus tube may be necessary, and if the disease is situated at the upper part, life might be prolonged by making an opening below the obstruction.

Stomach. The mortality from cancer of the stomach is very considerable. In this respect it yields to no organ but the uterus. Of the 8289 deaths in Paris, 2303 are referred to the stomach. In 67 cases, MM. Herrick and Popp found this organ diseased in 19. Cancer of the stomach may exist alone, but is usually associated with similar affections of other organs, more especially the liver. It is almost invariably primary. All the varieties of the three species of carcinoma occur in this organ; it is the special site of colloid cancer; and the milt-like variety of encephaloid is more common here than elsewhere. Infiltration is essentially the mode of deposition in this place, and, indeed, throughout the alimentary canal; and the seat of the deposit is the submucous cellular tissue, though the mucous membrane, particularly when hypertrophied, may become a nidus of formation. The pylorus is the part most commonly affected; next the cardiac orifice; then, the greater, and, lastly, the lesser curvature. Dr. Walshe has not met with an instance in which the disease was limited to the fundus—a fact of importance as bearing upon the supposed origin of the disease from the ingestion of irritants. Three-fourths, or even more, of the organ may be disorganized; and this is particularly the case when colloid is the species.

The mucous membrane long resists the disease, and its chief tendency is to become irregularly hypertrophous, giving rise to the apparent formations of vegetations, &c. The cellular structure undergoes very marked thickening, and this occurring between the muscular fibres produces the striated appearance so commonly observed. The muscular coat of the sound parts of the organ is often enormously hypertrophied. The peritoneal coat is seldom affected, excepting in cases of colloid.

The size of the organ varies extremely. When the pylorus is affected it is often greatly enlarged; when the cardiac orifice, it is contracted; when the body of the organ is alone diseased, the general bulk commonly remains unchanged. It is a curious and unexplained fact, that, where the pyloric orifice undergoes dilatation, the walls of the stomach become hypertrophous.

The progress of the disease presents nothing peculiar. Ulceration is slow to commence; but when once begun, its ravages are extensive. In most cases, adhesions, especially to the liver and pancreas, take place before the peritroneum gives way.

The disease is more common in males than in females, and between the ages of 35 and 60. It is often hereditary, and has apparently, in many cases, been induced by mental distress.

Cancer of the stomach, in the early stages especially, may be confounded with other affections; the most practically important of which are *gastrodynia* and *chronic gastritis*. We subjoin the following sketch of the chief points of distinction:

Gastrodynia.	*Chronic Gastritis.*	*Gastric Cancer (early period.)*
Tongue variable; but often pale, and pitted at the edges.	Tongue dry, red, contracted, smooth, shining, or saburral.	Tongue pale or natural.
Eructation frequent of air, without disagreeable smell.	Eructation not a prominent symptom.	Eructation of air more or less fetid, sometimes horribly so, a prominent symptom.
Appetite depraved, irregular, capricious.		Appetite diminished, or even totally suppressed.
Sensations, sometimes of heat, sometimes of cold in stomach; thirst not common.	Sensation of heat in stomach; thirst.	These symptoms not observed.
Solids more easily digested than liquids.		Liquids more easily digested than solids.
Digestion completed, though with much labour and suffering	Digestion imperfectly completed.	Digestion not properly effected.
Pain variable, occurs in irregular paroxysms; is often relieved by ingestion of food or pressure.	Epigastric pain not very severe, and scarcely ever felt when the stomach is empty; increased by pressure.	Epigastric pain may be agonizing; the lancinating character sometimes marked; often increased by pressure.
Epigastric pulsation not uncommon.	Is not observed.	Is not observed.
Never runs a completely latent course.	Never completely latent.	May for a variable time be completely latent.

Gastrodynia.	*Chronic Gastritis.*	*Gastric Cancer* (*early period.*)
Chronic vomiting is most frequent in females, and is almost confined to persons affected with hysteria.	Vomiting of sudden and severe character sometimes the very first symptom; occurs irregularly before or after eating.	Vomiting of sudden and severe character is never the first symptom; it occurs generally early in the morning, subsequently at variable periods after eating, or at periodical intervals.
Vomiting of coffee-ground-looking matter does not occur, unless from accidental and rare hæmatemesis.	Coffee-ground looking matter sometimes vomited; but this is rare and exceptional.	The matters vomited are at first glairy, then half-digested food, then coffee-ground or soot like.
Bowels generally constipated, but not obstinately so.	Irritation, colic, and diarrhea frequent, from extension of inflammation to intestine.	Bowels habitually and obstinately constipated; occasional severe diarrhea.
Febrile action accidental and rare.	Evening fever not uncommon.	Fever absent.
In females the chlorotic tint is often present.	Violet discoloration of the lips, conjunctivæ, face, &c., often present.	Straw-coloured tinge of skin may be obvious.
Often accompanied with various nervous or hysterical symptoms.	Not so attended.	Not so attended.
Hypochondria occasionally present.	Hypochondria not caused by chronic gastritis.	Hypochondria not an effect of gastric cancer. (?)
Is more frequent than the other two.	Is rarer even than cancer.	Is much rarer than gastrodynia.
Is more common in women than men.	Is probably equally frequent in both sexes.	Occurs more frequently in men than in women.
May exist in very young persons (e. g. æt. 15).	Occurs at all ages.	Is excessively rare before æt. 30.
Is often hereditary.	Is not hereditary.	Occasionally runs in families.
Is rarely referrible to any distinct local exciting cause.	Is often referrible to some distinct local exciting cause.	Is rarely, if ever, referrible to local agencies.
Is relieved or cured by stimulant, tonic, and anodyne treatment.	Is relieved or cured by antiphlogistic treatment.	Is not cured, but is relieved by special treatment.

As the disease advances the diagnosis becomes less obscure, but cases do now and then occur which present all the ordinary combinations of symptoms, and yet are not cancerous; so that the detection of *tumour* is the only absolutely certain sign. This is most easily discovered when seated in the pylorus or great curvature: it is much more difficult to detect when occupying the lesser curvature or the cardiac orifice. It must be remembered that the situation of the tumour changes continually; and it is of the last importance to bear in mind that the whole course of the disease may be of an intermittent character.

In regard to treatment but little can be said. Conium is the only supposed specific which is applicable. Dr. Walshe has derived benefit from a combination of trisnitrate of bismuth and extracts of hop, stramonium, and conium, in pill; and he believes that the treatment generally should be such as is applied to nervous, rather than inflammatory affections. Leeches and blisters may be used to relieve occasional local irritation. Opium is contraindicated by the constipation; but the Indian hemp may be tried. A drop or two of oil of cajeput on sugar is the safest carminative. Cold applications or a blister sometimes also relieve the flatulence, and a dose of morphia has been found useful in the same way. But most is to be done by regulating the diet, observing what agrees with the patient, making the *quantity* taken at each meal *small*, securing perfect regularity in the hours, and complete mastication of each morsel. Sickness may be relieved by effervescing draughts, prussic acid, blisters, rough ice allowed to melt in the mouth, the application of ice in bladders to the epigastrium, or an occasional dose of creosote. The bowels are best kept open by enemata: drastic purges are quite inadmissible.

M. Recamier narrates a case of pyloric tumour, "possibly cancerous," which was reduced in bulk by pressure, applied by means of a folded napkin secured by a bandage round the body. It may be well to try some such plan as this.

Intestines. The intestinal canal is among the more frequent seats of cancer. The Paris register gives 378 out of the 8289 deaths; in 221 of these the disease occupied the rectum, showing the much greater liability of the great than of the small intestines to affections of this nature.

Cancer of the small intestine is almost invariably primary: it is most commonly found in the duodenum and upper part of the jejunum, and is so rare in the ileum that its existence there has been altogether questioned. The symptoms are those of chronic intestinal disease complicated with evidences of increasing obstruction, passing eventually into a state of strangulation, but the actual diagnosis must depend upon the discovery of a tumour. The treatment is similar to that of cancer of the stomach.

Cancer of the rectum and anus are so well known, that we need not delay upon them. Dr. Walshe speaks in any thing but favourable terms of the operation for extirpation, and would limit its performance to those cases in which, from the completeness of the obstruction, speedy death appears inevitable, and when the use of bougies has failed to give even temporary relief. But even under these circumstances the surgeon has his option of making an artificial anus, either in the groin, in which case the peritoneum must be opened, or (according to Callisen's method, modified by Amussat) in the lumbar region, without injuring that membrane. For our own parts, we are inclined to give our decided preference to the last-mentioned operation.

Primary cancer is sometimes, but very rarely, found in the *mesenteric glands*. Dr. Carswell has shown that the disease may be seated on the free surface of the *peritoneum;* and the omenta, especially the *omentum majus*, is not infrequently affected, secondarily, with colloid.

The *post-peritoneal cellular tissue* is by no means an uncommon locality of cancer, and the tumours, which are usually composed of scirrhus and encephaloid in variable proportions, often rapidly acquire an enormous size without giving rise to any marked symptoms.

Cancer of the *spleen* is of so little practical importance that it may be passed by unnoticed.

The *pancreas* is among the rare sites of cancerous formation. It has been denied that the disease is ever of primary occurrence in this organ, but such an opinion is contradicted by well-ascertained facts, and, indeed, it would appear that its secondary affection is the least common. Both scirrhus and encephaloid are found here; the former is usually infiltrated, the latter is developed in the form of tumour. Generally a part only of the gland is affected, the head being perhaps the most frequent seat. The symptoms are obscure, and of course vary according to the influence which the diseased mass exerts over adjacent organs. There is usually pain; often vomiting; in many instances, but less invariably than has been thought, an abundant watery discharge from the mouth, which discharge is supposed, but not proved, to proceed directly from the pancreas; and, when the tumour has ulcerated, discharge of blood from the stomach and by stool. It should be observed, however, that the absence of such bloody discharge is no decided evidence that softening has not taken place.

Liver. The liver stands pre-eminent among all the organs of the body for the frequency of its cancerous affections—even the primary form is extremely common. And of all its organic diseases cancer is the most frequent; but, when occurring primarily, it is slow to contaminate distant parts. Encephaloid is found here in the pure cerebriform state, and in the solanoid and hematoid varieties; scirrhus chiefly in the chondroid and napiform varieties. Dr. Walshe has never seen colloid in this organ. The most common combination is scirrho-encephaloid.

When primary, cancers in the liver are usually limited in number; when secondary, they are often exceedingly numerous, and this is peculiarly the case when the stomach is the part first affected. The lobules appear to be the seat of deposition. Dr. Walshe has invariably found, on examining a section of the liver, adjoining cancerous nodules, with a common lens, that the surface is sparsely studded with minute milky, though somewhat transparent, spots, looking as if the discoloration, which softens off at the edges, were produced by the point of a brush. The lobules seem *infiltrated* with the milky-looking material, which is most abundant towards their centre. Their outline and size exhibit no obvious change.

Our author has never seen the general bulk of the liver reduced notably below the natural standard, but in some instances absorption of its substance proceeds so uniformly with the morbid deposition, that no enlargement occurs. In other cases the dimensions of the organ are greatly increased. The existence of the one or other of these states makes a vast difference as regards the facility of diagnosis.

The disease is more frequent in males than females, and is most common between the ages of 57 and 70. Many of the symptoms are the same as in cancer of the stomach: those which specially point to the liver are tumour in the hypochondria, jaundice, and ascites. When the tumour can be felt it is nodular on the surface: this distinguishes it from fatty enlargement. Jaundice, without ascites, occurs in somewhat less than half the cases. Ascites, without jaundice, is much less frequent. The two combined occur in a fourth or fifth of all cases; in about a third neither is present.

The treatment should be generally conducted as that of gastric cancer. Dr. Walshe thinks he has seen the progress of the disease stayed by liberal inunction of the iodide of lead ointment over the hepatic region, and the internal administration of liquor potassæ in infusion of taraxacum.

CANCER OF THE RESPIRATORY ORGANS AND APPENDAGES.

Passing by the cancerous affection of the *nares*, *frontal* and *sphenoid sinuses*, *epiglottis*, *larynx*, *trachea*, *bronchi*, and *bronchial glands*, all of which are noticed, more or less fully, by our author, we arrive at the *lungs;* and here we must delay for a little space, remarking *in limine*, that the reader will find in this section the best and most comprehensive account of the subject that we have anywhere met with. In 1091 post-mortem examinations of persons dying of all diseases, MM. Herrick and Popp found the lungs cancerous 6 times. In 67 cases of cancer these organs were affected 6 times. Of the 8289 cases included in the Paris registers, the fatal event is ascribed to the lungs in 8. The disease is most common as a secondary affection; when primary it shows little tendency to contaminate the system. Encephaloid is the species most frequently met with. The right lung is the most common seat of primary cancer; in the secondary affection both are usually implicated.

Secondary cancer generally exists in the nodular or tuberous form, more rarely as stratiform and cupulated sub-pleural patches; yet more rarely as a large irregular mass; and, least commonly of all, infiltrated through the pulmonary tissue. Primary cancer is found as an irregular mass in the majority of cases. It frequently occurs in the infiltrated form; very rarely in the nodular shape. It appears as the sub-pleural stratiform patch, or the sub-pleural nodule; is often both in the infiltrated and irregularly tuberous form in the same lung; and may exhibit itself as a series of fringed processes hanging from the free border of the organ.

All parts of the lung are liable to the disease, but the different forms affect a preference for particular localities. Thus, the disseminated nodular mass occurs scattered through the lung generally, but has a special tendency to accumulate near the pleura. The irregular tuberous mass is commonly found in the middle lobe of the right lung, whence infiltration extends in the majority of cases, though it may proceed from the apex towards the base, or *vice versâ*. It is probable that the intervascular interstices of the lobules are the main seat of formation. The secondary nodule doubtless originates in stagnation occurring in the ultimate radicles of the pulmonary vein. The progress of the disease is the same here as in other parts. The nodular form rarely produces any effect on the pulmonary tissue, except detrusion, with or without condensation, or atrophous rarefaction. A large tumour, of course, gives rise to condensation. Pneumonic consolidation is rare. Gangrene and pulmonary apoplexy have been observed. The ultimate branches of the pulmonary artery and vein are sometimes obliterated through extreme compression: in a case recorded by Heyfelder these vessels were converted into ligamentous cords from the heart onwards. It remains to be ascertained, whether, in such instances, a new circulating system is established round the affected parts, as Van der Kolk and Guillot have shown to arise in tuberculization. The trunk and larger branches of the pulmonary vein not unfrequently contain masses of the morbid deposit, and the same may occur in the superior cava. The bronchi undergo compression, or sometimes obliteration; or they may be partially destroyed by the extension of ulceration; or they are perforated by a portion of the morbid formation, which then vegetates in their interior. When a chief bronchus passes through the growth, and is there compressed, dilatation may take place above the obstruction. Little notice has been taken of the condition of the mucous membrane. The pleura is invariably in an unhealthy state. It is a fact of great practical significance, that in no case have phthisical tubercles, in any form or stage, been observed, and that ulceration of the small intestines has not been noticed.

The growth of cancer of the lung is not limited to any particular period of life, but the male sex seems to be more prone to the disease than the female. In 10 cases the mean duration of the disease (calculated from the first appearance of symptoms) was 13·2 months; the greatest 27· months; and the least 3·5 months.

In most cases the disease, when of secondary type, runs an entirely latent course, giving rise to no symptoms, physical or functional; but this is far from being the state of matters in the primary affection. Here pain is almost invariably present; there is usually dyspnœa, and always cough, which in some few instances is dry, but generally accompanied by expectoration. The matter thus brought up varies in quality; it may be simply mucous or catarrhal, or purulent or bloody. In the latter case the blood seems mixed thoroughly with serosity, mucus or muco-pus, and the sputa, thus composed, are opaque and commonly free from viscidity, though they may be somewhat glairy, and in colour may be pink, or resemble red or black currant jelly. Actual hemoptysis is of frequent occurrence. It existed in 10 out of 19 cases analysed by Dr. Walshe, and of the remaining 9, in 4 there was bloody expectoration. This is a very high proportion, 72 per cent., while M. Louis gives 66 per cent. as the ratio in the consumptive patients observed by him. The decumbency is generally on the back, with slight inclination towards the affected side; in some cases there is orthopnœa towards the close of life. We have also as important symptoms the effects of *centripetal* and *centrifugal pressure;* the former exhibited in dysphagia, from pressure on the œsophagus; distension of the superficial veins, general tumidness of the face and neck, prominence and staring expression of the eyeballs, lividity of the surface and œdema, from compression of the deep venous trunks; and stridulous respiration, weakness of voice, and labour in speaking, from pressure on the trachea. The latter is shown by expansion of the walls of the chest, but this only occurs when the disease is of the tuberous form.

Physical signs. These are well marked and most satisfactory, as will be seen from the subjoined tabular view:

(A) Simple cancerous infiltration, or infiltration associated with limited tuberous formation, the latter especially seated towards the root of the lung.	(B) Cancer of the lung mainly tuberous, but (it may be) associated with infiltration to a limited extent.
Inspection.	
Retraction or depression of the affected side, with more or less marked deepening of the intercostal spaces; diminished motions of expansion and elevation; diminished costal motions.	Diseased side expanded generally, but not with perfect uniformity, nor bulged inferiorly; intercostal spaces widened, flat, or even slightly fuller than natural; motions, both general and costal, completely abolished; fluctuation imperceptible in intercostal spaces.
Application of the Hand.	
Vocal and tussive fremitus diminished in intensity; impulse of heart distinctly perceptible over the anterior surface of implicated side in some rare cases.	Surface unnaturally smooth and even; vocal and tussive fremitus completely deficient; neither simple nor peripheric fluctuation; double pulsation sometimes transmitted from the heart.

Mensuration.

Semi-circular measurement natural or diminished; deficient increase of width during inspiration.	Semi-circular measurement of the side natural or increased; width of side unaltered by inspiration; antero-posterior diameter increased; vertical measurement increased in cases of very extensive formation; distance between the nipple and the median line greater than on the opposite side.

Situation of surrounding Parts.

Mediastinum may be detruded towards the opposite side; the heart may be similarly displaced laterally or downwards; the diaphragm and infra-jacent viscera are not depressed.	Heart and mediastinum detruded laterally; corresponding division of the diaphragm with the infra-jacent viscera more or less depressed.

Percussion.

Sound intensely dull and of short duration; resistance of walls strongly marked; special character of sound tubular in some cases about the lower border of the infra-clavicular region; the dullness of sound sometimes extends beyond the middle line.	Sound completely and extensively dull and of short duration; resistance of walls extreme; limits of the dull sound not altered by changing the position of the patient.

[Before softening of the cancerous matter.]

Auscultation.

Strongly marked, diffused blowing respiration, or, with the progress of the disease (in consequence of accidental obstruction or obliteration of a chief bronchus), respiration weak or almost suppressed,—retaining, as long as it continues audible, the bronchial or blowing special character, bronchophony; bronchial cough; heart's sounds audible with undue intensity over the affected part.	Diffused or tubular blowing respiration, intensely developed in some cases; rhonchi either absent, or those of co-existing bronchitis; bronchophony; bronchial cough; heart's sounds audible with undue clearness; double blowing murmur in very rare cases accompanying the cardiac pulsation above-mentioned.

[After softening and elimination of cancerous matter.]

Percussion. Sound may become somewhat clearer; acquiring at the same time more distinctly the tubular, almost amphoric, special character; the resistance of the walls may proportionally diminish.

Auscultation. Cavernous respiration; mucous, cavernulous, or cavernous rhonchus.

The unaffected lung is the seat of exaggerated respiration, and the percussion sound of the corresponding side may be fuller and clearer than natural.

We have been thus full in noticing the physical signs, because it is clearly by a correct appreciation of these that the physician will avoid errors in diagnosis: the original work contains many precise rules for his guidance, but we have not space to enter upon them.

The disease is necessarily fatal, and but little can be affected by treatment. Dr. Walshe has found the intense dyspnœa more relieved by large blisters, frequently repeated, than by any other measure.

We must refer to the original for an account of the little that is known regarding cancer of the *organs of circulation.*

CANCER OF THE KIDNEY.

It is only of late years that the subject has attracted much attention, but our author has succeeded in collecting forty examples as the basis of his description. When cancer affects the kidney the disease is rarely limited to that organ, and in the great majority of cases its implication is secondary. Primary cancer is, however, less uncommon than is generally fancied. The associated organs vary extremely, but

M. Rayer has shown that the liver and right kidney, and the adjacent parts of the stomach or descending colon, and the left kidney, are often affected together. It is less common than might be expected to see the bladder and kidney simultaneously diseased. Encephaloid is by far the most common species; scirrhus is decidedly rare, and colloid has not been detected in this locality. When the disease is primary it is disposed to pass through its stages rather rapidly; its advance is slow when secondary. The destruction of the renal substance and softening of the cancer forms cavities, filled more or less with variously altered blood and detritus. These cavities may remain shut sacs, or communicate with and pour their contents into the calices or ureter, or, in rare cases, into the extra-renal cellular tissue, the peritoneal cavity, or the interior of some portion of the intestine.

Encephaloid occurs in the kidney in the infiltrated and tuberous forms; the former more especially when the disease is primary, the latter when secondary. Both originate in the cortical substance. In some cases the disease affects the various parts of the organ pretty equally; in others the morbid matter accumulates, especially at the convex surface of the kidney, or in its upper half, while the lower part remains perfectly untainted. As the disease advances the organ loses its natural shape: Dr. Walshe has seen it large enough to fill three-fourths of the abdomen. The lymphatic glands and vessels are frequently implicated, and, by pressing on the duct, may give rise to great danger.

In 35 cases, the disease affected both organs 16 times; the right alone, 13 times; and the left, 6. It is most common in males, and in persons of advanced years, but may occur at any period of life.

In a few extremely rare instances the disease has remained latent until the patient's death; in others, pain of variable character has been the sole symptom throughout. Generally, pain is the only evidence of the malady until it has advanced so far that the tumour can be felt externally. The *direct* diagnosis depends upon the existence of this and another symptom, hematuria. The *tumour*, seated in the lumbar region, varies in size; it may scarcely protrude beyond the false ribs, or may reach the crest of the ilium; it may extend laterally beyond the umbilicus, and may obviously push forward the abdominal walls. In some cases it is distinctly visible and palpable in the lumbar region; in others there is no posterior intumescence. It is irregularly circumcribed, unevenly tuberous on the surface, unequally prominent, and unequally resistant in its different parts, variously elastic, but never really fluctuating. Whether the seat of spontaneous pain or not, it is almost invariably tender under pressure. Its dimensions are best ascertained posteriorly by the dull sound under percussion, which extends laterally to the flank; anteriorly, the amphoric note of the intestine is often producible over a limited portion of the dull sounding surface, from folds of intestine lying in front of the tumour—this is of importance in the diagnosis. The growth has usually been rapid.

In the early stages, the urine generally continues unaltered; but when the disease is advanced changes in that fluid are constant, and these consist essentially of the presence of blood or certain of its elements, of pus or encephaloid matter. *Hematuria* denotes either the breaking up of the tumour, or the coexistence of cancer in the calices or pelvis. It does not necessarily remain a continuous symptom, and the quantity of blood lost varies greatly. *Albuminuria* has been observed, but it does not persist for more than a few days after the cessation of hemorrhage. The discharge of *pus* is seldom very great; but the microscope would probably often detect the presence of encephaloid matter.

Retraction of the testicle is of rare occurrence. The digestive organs almost invariably suffer, and there is complete anorexia. The cachexia is not commonly highly developed. There is emaciation, and usually œdema of the lower extremities. General dropsy is rare; and it is a singular fact that the brain does not suffer, and the intellectual faculties often remain clear to the last.

Little can be done by treatment. The diet should be nutritious, unless there be hemorrhage. Astringents and iron are useful in warding off attacks of hematuria and the cachectic symptoms, especially general dropsy. Opium should be given to relieve the pain. Small local bleedings may be occasionally useful in the same way, and to relieve hemorrhage; but they should only be employed when milder means have failed. Counter-irritation can only prove injurious.

The diagnosis is sometimes exceedingly difficult, and mistakes have occasionally been made by even the most accurate observers. Our readers will find in Dr. Walshe's pages full directions for their guidance; by the accurate following of which the chances of error will be greatly diminished.

CANCER OF THE BLADDER.

Cancerous diseases of this viscus are much more frequent than is commonly supposed: 72 of the 8289 deaths, so often alluded to, were caused by it; and Dr. Walshe believes that if the term fungi, habitually applied to growths in that organ, were more correctly defined, the estimate would be much higher. Tuberiform encephaloid is the usual species, scirrhus in any form being comparatively rare. In most cases there is only one tumour; and the common seat is the triangular space between the orifices of the ureters and the urethra, and the parts immediately adjoining this space. It also occasionally springs from the fundus and anterior or posterior surfaces, but has very rarely been observed on the sides of the organ. The progress of the disease is the same as in other hollow viscera, but, as a general rule, ulceration is slow to occur. The bladder is generally contracted, but sometimes enlarged; its walls are usually hypertrophous, and the mucous membrane presents the columnar aspect. Evidences of cystitis are sometimes seen, and calculous disease frequently coexists. The prostate may or may not be similarly diseased, and the condition of the kidneys and upper part of the urinary passages is various; but it should be observed that the coexistence of primary cancer in the kidney with primary cancer in the bladder is, considering the intimate functional connexion of the parts, singularly rare. The primary disease is almost exclusively confined to the male sex, and does not appear to occur before the fortieth year.

Among the earliest symptoms is frequent and difficult micturition. Uneasiness, sometimes amounting to severe pain, and increasing after the urine is passed, is experienced at the neck of the bladder. In some cases there is complete retention, in others a full stream may be suddenly stopped. These variations are caused by the position and mobility or fixedness of the tumour. The urine is usually turbid, and often contains ropy matter, composed of pus modified by the evolution of ammonia from decomposed urea. Hemorrhage is frequent, often the first symptom, and occasionally the cause of death. In some cases tumour can be felt in the hypogastrium.

The diagnosis is most obscure, often impossible; but it is probable the microscope may afford assistance, by revealing the presence of cancer-cells in the urinary deposits.

The treatment should be merely palliative. To attempt the removal of the growths by operation would be to hasten the death of the patient.

CANCER OF THE GENITAL ORGANS.

The extreme inequality which prevails in the proneness of the two sexes to cancerous disease of these parts, and of the breast, is well shown in the following table, constructed from the Paris registers:

Of the Male,		Of the Female.	
Testicle	21 cases.	Uterus	2996 cases.
Penis	10	Ovary	64
Scrotum	7	Vagina	14
Prostate	5	Vulva	2
	43		3076
Breast	5	Breast	1147
	48		4223

Male Organs. Scirrhus is the species which most commonly invades the *penis.* A basis of this kind, however, not unfrequently gives origin to encephaloid vegetations. The disease may commence in almost all parts of the organ, but the glans and prepuce are by far its most common primary seat. It may originate either from a warty excrescence, or a pimple; or it may infiltrate the glans, or appear as a complication of venereal ulcerations. Phymosis, either congenital or acquired, is an exceedingly common accompaniment: and it appears probable that the irritation occasioned by this condition of the parts may act as an exciting cause of the disease in persons predisposed to cancer. Circumcision is, therefore, an advisable prophylactic measure, where the constitutional taint is known to exist. The duration of the disease is exceedingly various. It passes through the ordinary stages, and contaminates the lymphatic glands, the inguinal according to M. Buret, becoming first affected if the superficial parts are involved, the pelvic, if the deeper seated. Amputation is the only possible remedy, and it affords but a very miserable chance of cure. M. Lisfranc has proposed to dissect off the prepuce when this is the diseased part, leaving the rest of the organ intact; but the measure is one of doubtful utility.

The *vesiculæ seminales* have been found cancerous in some cases, but nothing is known of the symptomatology or diagnosis of such a condition.

The disease is rare in the *prostate.* When it exists it is usually of the encephaloid species. All the lobes of the gland may be affected, but the middle is specially liable. There is nothing peculiar in the symptoms, and the treatment must be merely palliative.

It is in the *testicle* that we find the most numerous examples of carcinoma, and here, too, encephaloid is the common species, true scirrhus being very rare. The growth, when of large bulk, often has the characters of a composite tumour: and yellow discoloration, usually ascribed to the association of tuberculous matter, but most probably produced by softened fibrin, is very frequently observed. Both testicles are prone to the disease, but there is no example on record of their being, either simultaneously or consecutively, affected in the same individual. In the great majority of cases the body of the testis is the part first involved; and it seems probable that the disease commences by infiltration of the intervascular interstices of the gland. The progress is the same as in other parts, but ulceration of the investing scrotum is slow to occur. The tunica vaginalis becomes early diseased; the arteries of the cord are greatly enlarged, when the tumour has acquired considerable bulk; and the subcutaneous veins exhibit the usual dilatation. Contamination of the lymphatic system invariably occurs, the lumbar glands being most commonly affected.

The lungs, liver, kidneys, brain, meninges, bones, especially of the spine, the mesenteric glands, vena cava, and heart, are among the observed seats of secondary cancer, when the testis is its primary nidus; and perhaps the implication of the brain and spine is slightly more frequent under these than any other circumstances.

The disease may occur at all ages; and it is here that external violence has been most observed as an exciting cause. Horse exercise should, therefore, be avoided by persons of cancerous descent.

Increase of bulk and hardness of some particular point are the symptoms first noticed. These advance progressively, until the organ may have acquired a large size, and have changed its natural ovoid for a spheroidal shape. The diagnosis is sometimes extremely difficult. *Simple hypertrophy* is known by its commonly affecting both testes; *simple chronic induration*, which in many of its symptoms closely resembles cancer, by its yielding to mercurial treatment. *Syphilitic induration* is accompanied by dull pain, which is generally worse at night, but never lancinating: the organ, though sometimes increased, is not unfrequently diminished in bulk, and, as it were, wrinkled, its investments retain their natural character; the density of the gland increases, and its suppleness and elasticity are lost. The history and effects of treatment will also help to correct error. *Tuberculous disease* scarcely occurs except in phthisical subjects. In the ulcerated state mistakes can hardly be made; in the chronic unulcerated condition, the diagnosis will be aided by the smaller bulk of the part, its more distinctly nodular character, and its extremely slow progress. *Fibrous tumour* is excessively rare; it might be distinguished by its great weight, and the absence of pain and constitutional symptoms. *Cystoid and cysto-sarcomatous growths* present great difficulties; but they grow more slowly, are not productive of pain, and do not affect the cord, lymphatic glands, or constitution.

Compression, and the internal use of the iodide of arsenic, combined with the local inunction of the iodide of lead ointment, is a plan of treatment which deserves full trial. The results of extirpation are melancholy in the extreme.

Female Organs.—We need not dwell upon cancer of the external genitals. The disease occurs in the *ovaries* either as a primary affection, as an extension or a sequence of the same morbid state in the uterus, or consecutively to the growth of cancer in distant and unconnected parts; but this is rare.

Scirrhus and encephaloid, both tuberiform and infiltrated, occur in this organ. The latter is the most common. Dr. Walsh has never met with colloid himself, nor found any satisfactory account of its being observed by others. The cancerous matter sometimes exists alone, but more commonly it is superadded to other morbid formations, and especially to multilocular cysts. Under such circumstances it is deposited in various forms on the walls of the cysts; but it is necessary to caution young observers against the frequent mistake of regarding as true scirrhous production, simple cysts the walls of which have been converted into fibrous tissue. The disease is usually confined to one ovary, though both are not unfrequently implicated. The morbid mass contracts adhesions with surrounding parts in many cases, and these adhesions have been found extensive where there had been no symptoms of peritoneal irritation—a fact of obvious practical importance. Of the special causes nothing is known, and the symptoms are merely those of ovarian enlargement in general. That it is no easy matter at all times to diagnosticate with accuracy the precise organ with which an abdominal tumour is connected, our readers are well aware; but we have not space to enter upon the question here. Let it then be granted that the growth, in any given case, is ovarian, how are we to determine its *nature?* If there be marked cancerous disease in any other organ, or if the cachexia be established, we have good ground for believing that the disease in question is of a similar kind. But should these helps be absent, we must be guided by the local characters of the tumour.

> "*Fibrous tumours of the ovary* may, when large, be distinguished from scirrhus by their size; from encephaloid by their hardness, inferior elasticity, smoother non-lobulated surface, and uniform consistence in every part. The condition of the patient's health may facilitate the distinction. *Unilocular cysts* are of globular outline and non-lobulated; they usually contain a serous fluid, in which case the fluctuation of this will readily prove that the mass of the tumour does not consist of cancerous matter; and even where the contents are of a gelatinous character, the phenomenon is sufficiently well marked to prevent mistake. They are frequently movable, and are of slow growth. The constitution suffers little in this disease. *Multilocular cysts*, on the contrary, either from the coexistence of their contents (firm, jelly-like matter, blood, fluid or coagulated, atheroma, acephalocysts, pilous and fatty substance), or from the non-communication of their cells, rarely give rise to more than very obscure fluctuation, which may almost be confounded with the elastic doughiness of encephaloid. Careful examination will, however, enable the observer to avoid this error.
>
> "It is not enough, however, that we be able to affirm that the entire mass of the tumour is not composed of cancer—an extremely rare occurrence; it is necessary to ascertain whether, and to what extent, encysted growths contain carcinomatous formations in their walls. In certain cases this may be done by the detection, both by manual examination and percussion, of irregular indurated masses contrasting with the softer divisions of the remainder of the tumour; when the cysts are exceedingly tense from thorough repletion with fluid, the sign of lobulation may not be ascertainable: in this state of things evacuation of the fluid by paracentesis will remove this difficulty in the way of the diagnosis. Nevertheless the discovery of these lobulated masses is not a certain sign of cancerous formation; bunches of sub-cysts, with gelatinous or albuminous contents (I mean as respects their outward appearance), aggregated in the parietes of larger cysts,

give rise to a precisely similar sensation. When hydatid fremitus exists in a marked manner, the presence of *acephalocysts* may be safely announced. Rapidity of course of the disease furnishes one of the best evidences of encephaloid complication."

In regard to treatment, Dr. Walshe strongly urges the full trial of iodine, both externally and internally, in the way before noticed. His own experience enables him to affirm that the progress of the disease may be thus arrested, and the size of the tumour in some cases actually diminished. In respect of puncture, it should be remembered that when there is encephaloid matter in the cysts, the evacuation of their fluid contents often occasions a more rapid deposition; the operation should, therefore, be undertaken only under the pressure of necessity.

We should now proceed to the consideration of the very important subject of *uterine cancer*, which is most ably treated by our author; but we have already wellnigh exhausted our assigned limits, and we deem it more advisable to employ what little space yet remains in bringing before our readers some points in the history of the disease which are less generally known; and we, therefore, purpose to pass entirely by this section and the next succeeding, which treats of *cancer of the breast*, with the simple remark, that in them will be found lucid descriptions of the morbid conditions, clear rules for diagnosis, and judicious advice in regard to treatment.

CANCER OF THE ORGANS OF INNERVATION AND APPENDAGES.

Encephalon. Every part of the encephalon has been the observed seat of cancer; but it is by far the most common in the cerebral hemispheres. Each of the three species is found here, but encephaloid much the most frequently. It exists in both the tuberiform and the infiltrated forms, the size and extent varying much. Generally, the brain contains but one cancerous growth. In some instances the tumours appear actually continuous with the surrounding brain; in others they are surrounded by a pseudo-cyst; while, most commonly, without such an envelope, they seem deficient in intimate connexion with the tissues around. The disease may be either primary or secondary. The state of the non-cancerous portions of the brain varies: it may be quite healthy; or exhibit punctiform injection, and flattening of the convolutions; or the neighbouring parts may be in a state of colourless softening. It is a singular fact that subarachnoid and ventricular effusion is usually very inconsiderable, and often altogether wanting.

The symptoms are unhappily obscure. Pain usually exists; but its characters, severity, and persistence vary within wide limits, and it is sometimes absent throughout. It is occasionally limited to a single spot, but is more frequently without defined position. The sensibility of the limbs and trunk may be diminished, increased, or perverted. Paralysis may or may not be present; it is sometimes hemiplegic, sometimes paraplegic, or it may be universal. It usually comes on gradually, but it may be developed at once. Partial or general convulsions are not uncommon. The intellect may or may not be affected. There is usually some derangement of vision, and deafness has been noted in some cases; but the senses of smell and taste rarely suffer. The nutritive functions remain long unimpaired, but eventually they fail, and the patient becomes emaciated. The skin rarely acquires the yellow tint.

Colourless softening is the affection most likely to be confounded with cancer. The marks of distinction, as stated by M. Durand-Fardel, are the less frequency of cephalalgia, its being rarely limited to one side of the head, and almost always frontal; the absence of complete amaurosis, and the extreme rarity of convulsions in non-paralysed parts.

But even when the evidence of a tumour in the brain is decisive, it may not be cancerous; for various other productions are found in the same situation. Experience has not yet given any sure marks of diagnosis; but it should be remembered, with reference to one form, the tubercular, that M. Louis has proved that, with the rarest exceptions, the presence of tubercle in any organ, involves, in subjects aged upwards of fifteen, its existence in the lungs. Careful examination of these organs will, therefore, render assistance.

The treatment can only be palliative; it should be directed to the removal of irritation in the brain.

When the *cerebellum* is the seat of disease, the pain is in some cases referred to the occiput and nucha, but this is by no means invariable. The intellect is rarely affected. In a very few cases only has there been defective co-ordination of muscular action, which ought to have been frequent had the doctrines of certain physiologists been founded on truth. Hemiplegia is excessively rare; paraplegia still more so. Convulsions are more common. Complete or incomplete amaurosis has been often observed. Deafness has never occurred, unless the portio mollis has been actually implicated; and the phrenological views of the functions of this organ receive no support from recorded cases of this disease.

CEREBRAL MENINGES AND CRANIUM.

Under this head Dr. Walshe includes those cases of intracranial cancerous disease, which are practically distinguished by the morbid production perforating the cranium, and forming a fungous growth on its external surface; as likewise those where it originates in the meninges, without making its way outwards through the skull; and those excessively rare cases, wherein the disease is alleged to have formed in the subpericranial cellular membrane. The whole chapter is one of extreme interest and value; but a mere glance must suffice us here.

The disease, which is usually encephaloid, but in some few instances true scirrhus, may originate in any one of the following situations: 1, the external or the internal lamina of the dura mater; 2, pia mater; 3, cerebral substance; 4, cranial bones, generally the diploe, but occasionally the compact tissue; 5, subpericranial cellular tissue. In most cases there is only one tumour, but sometimes they are very numerous. They have been found at all ages, and with nearly equal frequency in both sexes. The symptoms vary according as the tumour continues still within the cranium, or has made its way externally. Under the first-mentioned circumstances they are the ordinary evidences of compression, sometimes developed suddenly, but most commonly by slow degrees. When the bony covering has been perforated, which is effected either by carcinomatous opening of the osseous tissue, or, as is more usual, by atrophous absorption, the cancerous formation protrudes through the adventitious foramen in the form of a fungous tumour. This tumour may possess one or more of several characters, which, if combined in any number, are pathognomonic. It has the lobular surface and soft elasticity of encephaloid; it is immovable, yet non-adherent to the skin, which retains its natural appearance. The irregular edges of the perforation may sometimes be traced round it. It is the seat of two pulsations—one synchronous with the arterial pulse, the other corresponding with the rise and fall of the brain during respiration. It may be reduced either wholly or in part, and thus give rise to symptoms of compression; when left to itself, it recovers its previous appearance and size. It is not usually tender to the touch, but is often the seat of darting pain, which may be relieved by *gentle* pressure. Death usually occurs before ulceration has taken place.

But almost all these symptoms may be absent, and this constitutes the great difficulty in diagnosis. The disease may also be confounded with other maladies, as *encephalocele;* which, however, unless the result of accident, is almost peculiar to new-born infants—is, with the rarest exceptions, seated on the sutures or fontanelles, where cancer is not observed, is of equable consistence throughout, and passes through an opening with smooth and even edges. *Erectile tumours*, when they have attained any size, usually affect the condition of the skin, and their pulsations are more influenced by the general state of the circulation, and can be more readily controlled by pressure on the carotid. *Aneurism* will be known by the blowing murmur and the absence of opening in the skull. *Encysted tumours* and *abscess* could hardly be mistaken, unless the observer were very careless. *Cephalhæmatoma* exists in new-born infants alone; and only two cases of perforating fungus at this age are on record.

As respects treatment, Dr. Walshe recommends the trial of gentle compression. The lamentable results of surgical interference are strikingly shown in a table at p. 520.

Here we must pause. Our notice has extended to a much greater length than we had anticipated, and yet we feel that we have done our author but scant justice; and now we leave altogether untouched the very interesting and valuable information which he has collected regarding the cancerous affections of the spinal column and its contents, the nerves, skin, bones, and subcutaneous cellular tissue. To these, as well as all other portions of the volume, we again earnestly direct the attention of our readers; being confident that no where will they find a safer guide, or a more intelligent teacher than Dr. Walshe.

Art. II.

Die Gallige Dyscrasie (Icterus) mit acuter gelber Atrophie der Leber. Von Dr. P. J. Horaczek, practischen Arze in Wien, &c.—*Wien*, 1844.
On Bilious Dyscrasy (Icterus) with Acute Yellow Atrophy of the Liver. By Dr. P. J. Horaczek.—*Vienna*, 1844. 8vo, pp. 278.

This is an enlarged edition of a work that has been received with considerable favour; and in its present altered and amended form, is intended by its author, to serve as an introduction to the subject of biliary disease in general.

Dr. Horaczek points out the confusion and error that have been introduced into this important department of pathological inquiry by preceding writers, who have either confounded diseases essentially different under the same appellation, or have applied different names to diseases closely allied, if not actually identical.

He objects to the names icterus and jaundice as expressive of a symptom merely, and proposes to substitute for them the terms "bilious dyscrasy," "cholosis," or "cholomæa," as significant of the peculiar dyscrasy of the blood, and of its saturation with more or fewer of the elements of the bile, that constitute the essence of this group of diseases.

The disease he subdivides as follows: 1, primary, or idiopathic, or protopathic cholosis; 2, secondary, or deuteropathic, or symptomatic cholosis; 3, polycholia.

Primary, or idiopathic bilious dyscrasy, or cholosis, which is the subject of the work before us, immediately consists, the author tells us, of a diseased composition of the blood, and a contemporaneous disturbance or total suspension of the secreting functions of the liver; both being the result of a disaccordance or derangement of the due vital relations between the nervous and vascular systems. Various anatomical and physiological changes are constant attendants on these primary diseased conditions, of which the most characteristic is a diminution in volume and alteration in texture of the liver, which, under the name of *acute yellow atrophy*, he has made part of his nosological definition.

To primary bilious dyscrasy belong the greater number of rapidly fatal cases distinguished as jaundice, as well as many known under the names hepatitis, bilious fever, cephalocholosis, &c.

Secondary bilious dyscrasy originates in various structural changes of the liver itself or of the neighbouring organs, in consequence of which, bile is again taken up into the circulation after its formation, and the functions of the liver are mechanically impeded.

Polycholia consists in an inordinate formation of the constituent materials of the bile, and their accumulation in the blood, concurrently with undiminished or even increased functional activity of the liver.

The earliest and most important change characteristic of primary bilious dyscrasy is that which takes place in the blood. In consequence of a deficiency of fibrine, icteric blood coagulates imperfectly. The coagulum breaks down easily, and is of a dark yellowish brown or dirty red colour, little altered by contact with the atmospheric air. As far as can be ascertained by microscopic investigations, the blood-corpuscles are diminished in number, and swollen. They also vary from each other in shape, and are less clearly defined in figure than natural. The serum is opaque and viscid, and it, as well as the buffy coat, when the latter does occur, is deeply yellow from a large admixture of the colouring principles of the bile. The other biliary constituents are not found in the blood in cases of purely idiopathic cholosis; though the peculiar atomic combinations from which the liver prepares the bile, as well as the biliary acids, sometimes are. The peculiar atrophy of the liver involves shrinking to a half or even a fourth of its usual size, with wrinkling of its peritoneal coat. Its parenchyma is soft, flaccid, bloodless, and inelastic, of a greenish or dirty orange colour; and deprived of its peculiar granular structure, so that its different constituents can no longer be distinguished from each other. The gall-bladder is generally found contracted, and contains a colourless, or gray, or greenish slimy bile; its ducts are contracted and empty.

The spleen is in almost every case enlarged to twice or thrice its usual size, and easily lacerable or even reduced to a dark red pulp.

In severe and fatal cases the brain is found to have undergone serous infiltration, and great softening, especially round the central white parts. Yellow serum is found poured out as well over the convolutions of the brain and base of the skull as in the ventricles.

Anemia and relaxation of tissue or softening are found to be conditions common to the muscular, nervous, vascular, and glandular structures.

The mucous membranes in general, and the gastric mucous membrane in particular, are frequently softened. The mucous coat of the stomach, especially at the greater curvature, is found reduced to a dirty gray or greenish pulp. This change sometimes extends to the œsophagus, but more frequently to the duodenum; it is most frequently met with in the case of children.

Besides the foregoing, which Dr. Horaczek looks upon as constant and characteristic results of primary bilious dyscrasy, there are others of a casual or occasional character, of which the most important are congestions of the lungs, and passive inflammations of those organs or of the pleura or peritoneum; the products of which indicate the cacoplastic and diseased character of the blood.

The formation of coagula and limited inflammations in the portal or neighbouring veins, with consecutive purulent deposits in the liver, lungs, or other organs, are rare but very serious attendants on primary cholosis.

After having enumerated the symptoms which usually mark the onset of the disease, such as gastric and enteric disturbance, languor, lassitude, incapability of, or disinclination to, mental or bodily exertion, irregular chills and heats, loss of rest, depression of spirits, irritability of temper, &c.; the author enters at some length upon the consideration of the attendant febrile disturbance and characteristic yellowness.

The fever which seldom goes beyond mere irritative reaction, has generally an irregularly remittent character; the exacerbations assuming a regular quotidian or tertian type, only in situations favorable to the development of intermittent fever, or in the case of persons predisposed to neuroses of the ganglionic nerves. In many instances it disappears altogether after a few days. In the case of strong young persons, or those characterized by great vascular or nervous irritability, who have been subjected to the action of strong exciting causes, the disease often sets in violently with spasms or convulsions of an epileptic, tetanic, or hydrophobic character, or even with apoplectic symptoms; the attendant fever then wears an inflammatory aspect, with severe pain in the head, region of the liver, or abdomen, general turgescence, obstinate vomiting, red tongue, injected eyes, diminished secretions, heat, thirst, increased sensibility to light and sound, and full, hard, frequent, contracted pulse. After some days this high febrile action ceases, leaving behind a weak, soft, slow, and unequal pulse, or it may continue in a moderate form during the greater part of the illness, and terminate only with the general decline of the symptoms, or under imperfect critical efforts, manifested by general perspiration, deposits in the urine, bilious stools, or hemorrhage. The characteristic and never-failing discoloration which attends cholosis, extends over the surface sometimes in the course of a few hours, and also stains every organ and tissue in the body, except the substance of the brain and nerves, and the enamel of the teeth. It is obviously the result of vicarious efforts to get rid of the biliary elements accumulated in the blood, and varies from a clear or dark yellow to a green bronze, and even reddish hue, according to race, occupation, and the presence of other cachectic conditions. Thus, in cases of chlorotic complication it is of a grayish; in scorbutus, of a greenish or dark yellow tint. Other modifications of which it is susceptible, which have led to the adoption of distinctive appellations, such as icterus variegatus, ict. partialis, ict. dimidiatus, &c., generally belong to the secondary forms of cholosis, and depend on the coexistence of other dyscrasies or on organic disease.

The presence of the biliary colouring materials in the urine is shown by its characteristic saffron yellow colour when seen in thin layers, and also by its power of staining other objects, as well as by the effects of nitric and hydrochloric acids. If the freshly passed urine be carefully poured on a sufficient quantity of nitric acid, and the two be gradually mixed together, the red or yellow colour of the urine will pass into green, blue, and violet. If the colouring material in the urine be small in quantity, the change to green is succeeded by a restoration of the yellow colour, without the intervention of any intermediate tint. The yellow or brown colour of the urine, is also changed to green by hydrochloric acid, but less certainly and obviously; the smallest colouring admixture of the urine may also be detected by using a good achromatic microscope.

The other constituents of the bile, such as biliary resin, bilin, cholesterin, &c., are also present in icteric urine. The cholesterin is in some cases deposited, and may be detected under the microscope as regular prismatic crystals, or more frequently as an amorphous mass. Icteric urine, when voided, throws up a permanent froth, has a saponaceous feel, gives out a peculiar sickly sweetish smell, and deposits on the vessel a fatty opalescent scum.

When the disease is at its height the urine is alkaline, and disposed to putrefy; but as the symptoms decline it becomes acid soon after emission. The urea is also generally diminished, as well as the fixed salts and earthy phosphates. The specific gravity varies a good deal; it is often normal, but still more frequently falls to 1010-1008. These changes Dr. Horaczek looks upon as resulting from the diminished energy of the vascular system, and consequent imperfection of the respiratory functions, the effect of which is, that a smaller quantity of oxygen is brought into contact with the blood, and the decarbonization of its normal and abnormal constituents is diminished; and hence, as in the case of the amphibia, the formation of urea is lessened, and that of uric acid is increased.

The taste and reaction under nitric acid show that the biliary constituents are present in some of the other secretions, amongst which may be mentioned the perspiration which tinges linen—the serum, whether of the blood or that poured out by blisters, or from œdematous parts—the various mucous discharges, and those from abscesses and ulcers.

The paucity of observations does not allow us to decide whether the tears, saliva, semen, and milk, undergo similar changes. But the bitter taste of the milk observed by some, and the refusal of the breast by the infant, show that it must have sustained some qualitative change.

The characteristic pain of the liver, which sets in at the commencement of the disease, and accompanies it throughout, is of a nervous character, and is generally referred to a circumscribed spot over the left lobe. In slight cases it amounts only to a feeling of tension and oppression, with occasional fugitive stitches; aggravated by pressure so as to compel the patient to make a forced respiration. In bad cases it becomes very severe. It goes hand in hand with the loss of function and decrease in size of the organ.

The increase in volume of the spleen, which keeps pace with the atrophy of the liver, is evidenced by feelings of tenderness and oppression in the left hypochondrium, which sometimes increase to severe pain obstructing respiration.

There is also a remarkable pain in the eighth or ninth dorsal vertebra; as well as pains in the limbs and joints of a rheumatic character, which alternate with a feeling of stiffness, and a consciousness of stupor, or total loss of sensation in the parts. As these painful affections are witnessed in cases where there has been violent vomiting; Dr. Horaczek thinks they may be attributed to the mechanical agitation of the body. Embarrassed respiration and constriction of the chest, which are increased at night, or on making any sudden movement, and sometimes induce fits of palpitation, he attributes to the diseased condition of the blood. Disease of the heart or lungs inducing similar symptoms may be detected by percussion and auscultation. The advanced stages and severer forms of the disease are marked by cephalic and nervous symptoms, which set in sometimes quite suddenly and unexpectedly. To restlessness, sleeplessness, high delirium, repeated vomiting, and tonic or clonic spasms, succeed apathy, diminished sensibility to external impressions, low muttering delirium, with rapid, feeble, unequal pulse, gradually increasing, and at length profound coma, paralytic affections, total unconsciousness, involuntary evacuations, cold viscid sweats, and death. This modification of the disease has been named nervous jaundice, or cephalocholosis. Sometimes the disease runs on to regular typhus, or it ends in dysentery, or the powers sink under coffee-ground vomiting and purging, indicative of passive hyperemia, and softening of the mucous lining of the intestine canal.

Of combinations and complications, besides those just noticed, the most serious is that with inflammation of the abdominal veins, which the author thinks may be attributed to the rapid shrinking of the liver inducing retarded circulation and partial coagulation of the blood. The coagulum so formed undergoes a purulent decomposition, and induces inflammation of the inclosing vessels, attended by the usual symptoms. One very important remark, especially in a practical point of view, is, that genuine healthy inflammation is incompatible with cholosis; though inflammatory congestion with softening of the mucous and serous membranes, and passive inflammations of the lungs and other parenchymatous organs are frequent enough.

All these processes are, however, slow and irregular, without any tendency to a critical termination, and partake of the nature of mere congestion and infarction, with a disposition towards decomposition rather than to proper inflammatory plastic processes. Whilst alluding to certain periods of life as predisposing causes of cholosis, he takes occasion to notice icterus infantum, respecting which he seems to doubt what may be its true relation to biliary disease in general. He thinks it probable that many cases belong to primary cholosis, both from the symptoms during life and appearances detected after death. Some may be referred to secondary varieties of the disease, but there are not a few whose true circumstances and position remain to be determined by numerous and accurate observations. With respect to progress and duration, he defines the disease to be ("atypische") one *sui generis*, irregular in progress, undetermined as to limits, and of uncertain duration, which develops itself gradually or suddenly.

In some cases the symptoms disappear suddenly before they have become more than precursory, or they are so mild as scarcely to claim medical interference, or interrupt the patient's usual avocations.

The average duration of the disease is from four to six weeks. When prolonged beyond eight or ten weeks, it passes into the secondary forms. It may, however, destroy life in eight or ten days. It is not the subject of proper critical efforts, for although a certain degree of alleviation sometimes follows deposits in the urine, general perspiration, discharges of blood, eruptions on the skin, and the restoration of suppressed secretions or excretions, yet Dr. H. does not consider them properly critical, because they do not appear at any regular period, are only accidental, and the disease often terminates favorably without them. Bilious stools frequently bring about a very decided improvement, but they have not always such a desirable result, nor is the time of their appearance determined; and, therefore, even they cannot be strictly called critical. The prognosis is generally favorable. Bilious dyscrasy sometimes, though not often, ends in other diseases; generally in bilious cachexia, with a disposition towards the reappearance of the original malady, or towards the development of its secondary forms. In such cases we have digestive derangement, chronic vomiting, irregular action or torpor of the liver, disposition towards the formation of gall-stones, shrinking and hardening of the liver, &c. It may also end in putrid fever; and if the brain has been much engaged, it may leave behind epilepsy, maniacal affections, or paralysis of the extremities.

When death does take place, Dr. Horaczek views it as the result of poisoning and paralysis of the cerebral nervous system, evidenced by the cephalic symptoms before enumerated, which follow the derangement or disturbance of the mutual relations between the nervous and vascular systems, and the subsequent softening of the organs chiefly implicated. Chemical analysis of the blood, and of the various secretions and excretions, is the most suitable, and, with the improvement of chemistry, will be the most certain method of diagnosticating cholosis.

We may thus determine whether cholosis, anemia, cachexia following the abuse of spirituous liquors, that which attends cancer, pyæmia, or purulence of the blood, phlebitis, &c., all of which are accompanied by more or less yellow discoloration of the surface, are also complicated with cholosis or not.

It is often difficult to distinguish primary cholosis from some of its secondary forms, dependent on disease of the liver, pancreas, pylorus, duodenum, pleura, &c., or on obstruction of the biliary ducts, feculent accumulations in the colon, or pregnancy. Disease of the hepatic parenchyma, attended with increase of volume, may, however, be detected by manual examination. There is, besides, in such cases emaciation, with long-continued dyspepsia and dropsical appearances; the yellowness, moreover, is earthy, the skin is dry, harsh, and faded, and the symptoms indicating the narcotising effects of the accumulated biliary constituents on the nervous system are not well pronounced.

It is difficult to diagnosticate jaundice from gall-stones, from primary cholosis, especially if it attack young persons under the influence of mental emotions, and with acute symptoms, such as vomiting, pain in the liver, &c. Gall-stones are, however, generally a disease of persons advanced in life; the pain they cause is colicky, attended by a sensation of burning heat, sometimes remitting, or coming on at intervals like the pains of labour, and it often ceases suddenly, or is relieved by remedial agents. There are also cramps of the abdominal muscles, and of the whole body; the skin is covered with cold sweat, the pulse is contracted, the head is not engaged, and inflammation of the liver or neighbouring parts often succeeds.

Polycholia cannot readily be confounded with primary cholosis, as it is attended by increased size and increased functional activity of the liver, together with an overflow of bile into the intestinal canal; as well as by an increased formation of biliary matter in the blood, and jaundice, consequent on its insufficient separation through the liver. To polycholia belong bilious fevers, and bilious pneumonia and pleurisy.

It remains to notice some of the chief therapeutical indications according to our author, as well as the means by which he proposes to work them out.

The causes of the disease, if still in activity, must if possible be removed, or their operation be neutralized.

The restoration of the disturbed relations between the functions of the vascular and nervous systems he lays down as another indication. In reference to which he remarks truly enough, that as the precise nature of this disturbance, not less than of the normal relations of these functions to each other, is and ever will be a mystery, we can only resort to such rational or well-devised empirical measures, as tend to restrain their activity when in excess, and to excite it when unduly depressed.

We are as yet acquainted with no agents calculated to neutralize the injurious effects of the biliary elements on the blood, except those found to be useful in other cachectic and dissolved conditions of that fluid; of which the best are the mineral and vegetable acids, and preparations containing chlorine. The most important indication is to eliminate the biliary admixture from the blood, by exciting the activity of the liver; for this purpose purgatives must be used, of which he prefers rhubarb and aloes in full doses. Calomel must not be given except in occasional and purgative doses, lest the plasticity of the blood be still further reduced; and for the same reason the author objects to saline aperients, which merely increase the intestinal secretions, except in those cases where it is desirable to make the intestinal canal the seat of a vicarious discharge of the biliary elements, in consequence of a total and persistent suspension of the functions of the liver.

If we are unable to restore the functional activity of the liver, the best substitute for it will be excitement of the kidneys by means of diuretics.

If symptoms of biliary oppression of the brain display themselves, the restoration of the hepatic functions must be attempted by strong emetics, drastic purgatives, dry cupping, stimulating applications to the abdomen, and stimulating enemata.

If the cerebral oppression be caused by hyperemia, or an inflammatory state of the brain, which is sometimes, though rarely, the case, we may resort to local bloodletting, cold applications to the head, sinapisms to the feet, or stimulating pediluvia, repeated doses of tartar emetic in decoction of tamarinds, and cooling acid drinks.

If the cerebral affection be purely nervous, besides external stimulants and revulsives applied to the head itself, as well as other parts of the body, the author resorts to the internal use of stimulants and tonics, enumerating in addition to those generally used in these countries, phosphorus and the flowers of the arnica, which last he prefers to all others, on account of its specific effects on the brain.

Pain in the liver, which is almost always purely nervous, he treats with external emollients, derivatives, and sedatives.

He subjoins a tolerably extended list of popular and superstitious remedies, to which additions, equally whimsical and disgusting, might be made from sources nearer home.

The book concludes with a detailed narrative of upwards of 90 cases. More than one third of the number terminated fatally, and the morbid appearances which they presented on post-mortem examination are also given, so that we are furnished with copious illustrations by which to test the doctrines and precepts inculcated by the author.

We here conclude our account of Dr. Horaczek's work, from which we have extracted largely. Although the author's pathological views are not always very definite, and are sometimes very open to criticism, we still think the book is well worthy of a careful perusal, as it supplies an important addition to our knowledge of a very obscure class of diseases, which it presents under aspects chemical, pathological, and even semeiological, in some respects both new and important; and induces us to look with considerable interest for the fulfilment of Dr. Horaczek's intimated design, of giving a complete view of the subject, by the publication of his researches on the other forms of bilious dyscrasy.

ART. III.

Transactions of the Provincial Medical and Surgical Association. New Series. Vol. II.—*London*, 1846. 8vo, pp. 285.

THE contents of the present volume are the following:—1, The Retrospective Address in Medicine, by Edward Charlton, M. D.; 2, The Retrospective Address in Surgery, by Thomas P. Teale, Esq.; 3, On Grinders' Asthma, by C. F. Favell, M. D.; 4, On the Inverted Displacement of the Urinary Bladder, by J. G. Crosse, Esq.; 5, Report of the Reading Dispensary for the years 1841–4, by Charles Cowan, M. D.; 6, A Statistical Report of the Surgical In-patients of the Royal Berkshire Hospital from 1839 to 1845, by George May, Esq.

I. Our readers have had so much from ourselves in the form of Reports of the progress of medical science in its different branches, that they will excuse us for not giving any account of the two excellent addresses with which the volume opens. We must also content ourselves with referring to the original report of the Berkshire Hospital and Dispensary, which do great credit to the industry and talents of their authors. It is surprising, when we consider the great number of hospitals scattered over all our counties, that so few of their brethren have emulated Dr. Cowan and Mr. May in enriching the pages of these transactions with similar documents.

II. Mr. Crosse's paper gives the history of a case of rare occurrence, and is important, as exhibiting the value of a careful examination and consequent just diagnosis, and the dreadful risk attending a mistake in this particular. We extract the more material points of the narrative:

"In the year 1829, a highly respected colleague of mine, since deceased, received under his care a healthy-looking female child, aged between two and three years, on account of a tumour, about the size and shape of a walnut, projecting visibly at the external *labia pudenda.* It was of a florid red colour, and somewhat granulated upon its surface, so as to resemble a large strawberry; and the surgeon entertained a notion that it was a vascular tumour, which might be removed by ligature, on which account he requested me to inspect it.

"After a slight examination, I expressed my doubts as to its being a vascular tumour, and dissuaded him from the hasty application of a ligature. I could not, however, immediately explain its nature, having no conception how such a tumour could be formed by the displacement of parts only, without any superadded morbid growth. Towards the posterior part of the tumour, and on its sacral aspect, there was an aperture, which was conjectured to be the entrance into the displaced urethra. A very small female catheter easily entered this aperture, and passed along a channel a little to the left side of the median line: urine distilled in drops through the catheter, but there was not a gush, although the instrument had entered so far that we concluded it must have reached the cavity of the bladder. Besides what thus oozed through the catheter, slightly tinged with blood, there was an oozing of urine from another source, which was not explained until a second and more strict examination, instituted a few days afterwards, on my casually coming to the patient's bedside, just as the surgeon was prepared to apply a ligature round the neck of the tumour.

"I now found, concealed in a fold of the tumour, and near to the posterior junction of the labia, two orifices not far asunder, from which the urine oozed, and which were evidently the vesical terminations of the ureters. On pressing the tumour firmly, as if to reduce it like a hernia, I found it yield and pass gradually behind the symphysis pubis, and within the labia; and under a continuance of the taxis it all retired, leaving the external parts in their proper shape and position. A passage remained, through which the tumour on retiring had taken its course, which was actually the dilated urethra, into which I *could* and *did* introduce my little finger, until it fairly entered the cavity of the replaced bladder; for it now became clearly demonstrated that the vascular red tumour, externally presenting itself as first described, was the urinary bladder in its entire thickness, including its mucous, muscular, and peritoneal coats, prolapsed through the dilated urethra, and at the same time inverted or turned inside out. The proper lining membrane of the bladder became, in the progress of this displacement, the external covering of the tumour. As fast as the urine was secreted by the kidneys, it oozed from the terminating orifices of the ureters, which were concealed within a fold of the exposed surface of the tumour, and approximated to each other. The neck, or deepest and narrowest part of the tumour, just concealed within the labia, was covered by the inverted lining of the urethra, the inversion being complete.

"In this instance, had a ligature been efficiently applied to the neck of the tumour, as was contemplated, the bladder would have been removed, including all its coverings, the ureters cut through just above their terminating orifices, and the peritoneal cavity largely opened, with a necessarily fatal result!

"As the friends of the child could not be applied to, the history was imperfect. It was stated that the tumour had existed for a considerable time, and been always attended by stillicidium urinæ; also that it had been once replaced, but descended again, shortly before it came under my observation. During the short period that the child remained under my notice, after the replacement of the bladder, there was no relapse; and since this account was sent to press, I have been fortunate enough to ascertain, and to be enabled to add, that the patient is still living, after an interval of sixteen years, and is a healthy young woman, save only the affliction of the incontinence of urine, with which she has been constantly troubled, but without any relapse of the vesical displacement.

III. We had occasion, some two years past,* to draw the attention of our readers to the disease familiarly known as Grinders' Rot or Asthma, in connexion with the work of Dr. Calvert Holland. The tolerably full notice we bestowed upon that work will save us from the necessity, in the present place, of entering into any preliminary details concerning the various modes of occupation and pursuit of the artisans who fall victims to this singular affection—singular, perhaps, rather in its peculiar mode of manifestation, than in the fact that some disease or other of the pulmonary organs should be the result of the mode of working pursued in Sheffield and its vicinity. Besides, much fuller (and, as far as may be judged from internal evidence, more correct) information on this branch of the subject may be derived from Dr. Holland's volume than from the essay now before us.

"The object which I have peculiarly in view, in the present communication," says Dr. Favell, "is to determine the pathology of grinders' asthma, or rather, perhaps, I should say, to exhibit the lesions of the respiratory organs, which morbid anatomy most frequently reveals in the persons of those who have fallen victims to this disease."

Here two things most different—the pathology and the morbid anatomy of a disease—are spoken of as though they were all but one and the same. We notice this as a mere verbal inaccuracy. The author knows as well as we do, the difference between them.

---

* Brit. and For. Med. Rev., October, 1844.

Dr. Favell says that the most remarkable physical signs observed in these cases are "more or less imperfectness of expansion in some portion of the chest, an abnormal amount of dullness on percussion, the substitution of the tubular for the vascular murmur, and the occasional existence of amphoric resonance, cavernous respiration, and pectoriloquy." Now Dr. Holland, on his side, puts forth the following statements:—"The chest generally sounds well on percussion," "far better than would be anticipated from the pulmonary affection;" and, further on, the sound on percussion is described as "being frequently much louder than in health." The respiration is, according to the same writer, natural, puerile, or bronchial. Doctors will differ; so that there is nothing more than an amiable deference here to the common opinion, which hath made itself known in the well-worn saw. But if the cause of the difference be sought for, we are, we confess, puzzled. Can any of the "grit," or the "dust," or the "flying particles," so common in the locality, have obtruded itself into the ears of either observer, and transformed the sounds actually evolved into others harmonizing more or less distinctly with the theories held by each? But whatever be the explanation, the fact is a most singular one.

"Sensual gratification is the great object the grinders have in view,"—a character which is in nowise peculiar (as Dr. Favell's inference would seem to be) to the class of whose pulmonary diseases, he has composed a history. Nor do we, as a general fact, descry the utility or philosophy of seeking to enrol debauchery, venereal or spirituous, among the "causes" of the grinders' rot, when it is clear as the light of heaven, that if the poor wretches led the chaste and temperate lives of very anchorites, and continued to inhale this pernicious grit, they would perish victims of the "rot" its inhalation engenders. We have no doubt that spirituous drinks and consequent inebriety do greatly aggravate the local malady of these miserable men; but, at the same time, we say that it might be asserted with almost as fair a show of probability, and for any thing shown to the contrary by the historians of the "rot," that habits of inebriety act rather in the direction of slackening than accelerating the speed with which the pulmonary disease runs its fatal course. This may appear a paradox to some pathologists (we mean those of the *à priori* school of reasoning),—but we profess to *observe*, and not to frame opinions for one sect or the other.

Dr. Favell relates some cases of the disease with the particular view of illustrating its morbid anatomy. From these cases it appears that the main morbid changes discovered are—"1, tubercles; 2, small bodies resembling currants disseminated extensively on the surface and throughout the substance of the lungs; 3, large masses found in different portions of the pulmonary tissue; 4, emphysema; 5, dilatation of the bronchial tubes; 6, inflammation of the lining membrane of the bronchi, trachea, and larynx; 7, adhesion of the pleuræ; 8, enlargement of the bronchial glands; 9, enlargement of the heart; 10, a granular condition of the kidneys."

The disease of the kidneys and the adhesions between the pleuræ the writer regards as intercurrent affections, and not necessarily connected with the disease of the lungs. He very correctly shows the error of Dr. Holland's notions concerning the state of the bronchial glands and dilatation of the bronchi as constituting peculiar phenomena in this disease.

The primary or essential changes are inflammation of the larynx, trachea, and bronchi, small bodies resembling currants, and tubercles. The laryngeal and tracheal inflammation, with occasional ulceration, is said by Dr. Holland to be in a large number of cases the primary affection—he holds that it remains sometimes for a considerable period. This Dr. Favell "doubts." But Dr. Holland says, that on examining "the larynx during the continuance of these symptoms, the mucous membrane is often much more florid and vascular than is natural, and occasionally small ulcerated points are observed." And now behold Dr. Favell has Dr. Holland on the hip, and, having him there, is somewhat merciless towards his colleague and townsman: "Now, this statement I shall not attempt to contravene, inasmuch as I do not pretend to be able to inspect the lining membrane of the larynx during the lifetime of the patient." Dr. Favell is evidently sharper than Dr. Holland; his sidelong cuts at the rival historian are remarkable enough. Indeed, if we may judge from Dr. Favell's exhibition of temper, it would seem probable that the existence of harmony scarcely prevails amid the doctors of Sheffield; and we cannot but think it would have been more dignified and decorous in the author to have shown a less critical spirit in dealing with the opinions of his townsman, more especially in a paper publicly read in an assembly of their *local brethren.*

The "currant-like bodies" Dr. Favell considers are nothing more than the dilated extremities of veins, containing some of the solid constituents of the blood. He says that he has traced such appearances with the scalpel. Were we disposed to be critical, we might fairly question Dr. Favell's being any more able to trace the *extremities* of veins with the scalpel, than Dr. Holland to see the interior of the larynx in the living man; but we understand both, and we merely advert to the circumstance from the natural suggestion of stones and glass houses.

Dark masses as big as a nut or as an orange—sometimes grayish, sometimes black—sometimes dense, sometimes easily cut, are found in some cases. What are these? "In some instances I believe they are occasioned by the effusion of blood into the parenchymatous substance of the lungs, constituting what is properly called pulmonary apoplexy; but in the majority of cases they are doubtless the consequence of pneumonia, either in the acute or chronic form, and to which the grinders are peculiarly liable." Now this "sometimes one, and sometimes the other" mode of getting rid of a difficulty, reminds us of the well-known answer of the Cantab. at the "Little-Go," who to the question of his good-natured examiner, as to whether the sun moved round the earth, or the earth round the sun, boldly replied (by way of making sure) "sometimes one and sometimes the other." Dr. Favell must surely have felt that as two states could never be more dissimilar in nature and pathological signification than hemorrhage and inflammation,—yet he makes them have almost convertible morbid changes.

On the whole this essay gives us a rather more precise account of the errors and inadvertences of Dr. Holland than of the real nature of the disease, concerning which it professes to enlighten us. It, however, exhibits talent; and we are glad to receive it as an earnest of something better on the same subject from the same pen. The author's general conclusion is that "the disease essentially depends on congestion, or inflammation of the parenchymatous structure of the lungs; in some cases giving rise to the formation of tubercle, and in others occasioning pulmonary degeneration without tubercular deposit."

ART. IV.

1. *Pathologische Anatomie des menschlichen Körpers.* Von JULIUS VOGEL. Erste Abtheilung. (Allgemeiner Theil.)—*Leipzig*, 1845.

The Pathological Anatomy of the Human Body. By JULIUS VOGEL.—*Leipsic*, 1845. pp. 533.

2. JULII VOGEL *Icones Histologiæ Pathologicæ. Tabulæ Histologiam Pathologicam illustrantes. Viginti sex Tabulæ, continentes* CCXCI *Figuras, quarum* CCLXX *ad naturam delineatæ sunt.*

Erläuterungstafeln zur pathologischen Histologie mit vorzüglicher Rücksicht auf sein Handbuch der pathologischen Anatomie, herausgegeben von Dr. JULIUS VOGEL, ausserordentl. Professor der Medizin in Göttingen. *Sechs und zwanzig Tafeln mit* 291 *Figuren, wovon* 270 *nach der Natur gezeichnet sind.*—*Leipzig*, 1843.

Illustrations of Pathological Histology, illustrative of his Manual of Pathological Anatomy. By Dr. JULIUS VOGEL, Extraordinary Professor of Medicine at Gottingen. *Twenty-six plates, with* 291 *Figures, of which* 270 *are drawn from Nature.*—*Leipsic*, 1843. pp. 120.

DR. JULIUS VOGEL has long been known as an accurate chemist, a good microscopic observer, and a sound pathologist,—qualifications which eminently fit him for the duty he has now undertaken of publishing a treatise on morbid anatomy. In addition to the works whose titles head this article, he is favorably known as the author of essays "on the Sputa in various Diseases," and on "the Physiology and Pathology of Pus," of the chemical portion of Wagner's 'Elements of Physiology,' of an "Introduction to the use of the Microscope in the Chemical Analysis of Animal Matters" (reviewed in Vol. XVII, p. 424), and of the articles "Inflammation," "Morbid Tissues," and "Hypertrophy," in Wagner's 'Cyclopædia of Physiology,' now in the course of publication.

In the introduction to the present work we find some excellent remarks on the proper means of acquiring a knowledge of pathological anatomy, and as we believe that these means are neglected, and indeed, despised, by some of our English pathologists, we avail ourselves of this opportunity of pointing out to them what are deemed by their German brethren the qualifications requisite for the successful cultivation of this department of science.

"The study of those morbid changes to which the various parts of the body are

> liable, depends on a thorough previous knowledge of their normal relations; hence pathological anatomy requires a perfect knowledge of the structure of the healthy body, and of the uses of its different parts, in order to be able to estimate the influence which any morbid alteration of an organ impresses on its function. We must not content ourselves with studying the coarser changes, such as are visible to the naked eye, but we must examine those finer modifications of the tissues which can be recognized only by the microscope; hence the necessity for an accurate knowledge of general anatomy or histology. In the investigation of delicate points connected with pathological histology the microscope is altogether indispensable, and the application of chemical reagents must be observed under it. Chemical analysis is indeed of the greatest importance in pathological anatomy, being the only means by which we can on several points obtain the desired information." (p. xxxii.)

The whole work is divided into two parts,—the general and special. Of these we have at present only the former, the latter being promised in the course of the present year. The Icones, which were published about three years ago, refer both to the special and general department, there being twelve plates illustrative of general, and fourteen of special pathological anatomy.

GASES. The first chapter is devoted to the consideration of the *abnormal development of gaseous matters.* Accumulations of gas within the body may arise from very different causes.

1. *They may be produced by the external pressure of the atmospheric air.* The mechanism of this form of origin is most strikingly seen in those cases of general emphysema which arise from an injury to the lungs dependent on a penetrating wound of the thorax. Our author gives the following graphic sketch of the progress of the affection.

> "The air admitted into the cellular tissue of the thorax gradually works its way over the body, and the emphysema thus becomes more or less general. The orbits become closed up; the eyes and mouth remain shut, in consequence of the swollen condition of the eyelids and lips; the nose is hidden between the tumid cheeks; the skin of the neck is so monstrously distended that all distinction between the head and neck disappears; the scrotum swells to such a size as to conceal the penis; the limbs enlarge and assume a cylindrical form; the palms of the hands and the soles of the feet (in consequence of their firm connexion with the subjacent tissues) being the only parts not affected. In unfavorable cases the patient dies from impeded respiration and apoplexy, in consequence of the compression exercised on the air-tubes and jugular veins by the swelling." (p. 2.)

2. *Gases may be developed in the body in consequence of decomposition, fermentation, and putrefaction.* In accordance with the laws of organic chemistry, most animal and vegetable bodies undergo decomposition at the temperature of the human body, and in the presence of moisture, even when air is excluded. Gases are thus developed, which vary in accordance with the putrefying substances that give origin to them. Non-nitrogenous substances yield carbonic acid, carburetted hydrogen, and hydrogen; nitrogenous matters yield ammonia in addition to carbonic acid, and if sulphur and phosphorus are present, sulphuretted and phosphoretted hydrogen, and hydrosulphate of ammonia are also developed. Gas is thus developed in the human body from two distinct sources,—from food in the intestinal canal, in the act of decomposition, and from the decomposition of the

tissues of the body itself. The gases produced in the intestinal canal occasionally permeate through its walls into the peritoneal cavity. The slate-gray colour of the surface of the spleen and liver frequently observed in *post-mortem* examinations is due to the action of sulphuretted hydrogen or hydrosulphate of ammonia which must have escaped in this manner.

This chapter concludes with a few observations on the disputed question —*whether different parts of the body can actually secrete gas.* On this point the author offers no decided opinion.

DROPSIES. The second chapter is devoted to the consideration of dropsies, which are divided by our author as follows:—1. *Serous dropsy*, or that in which the fluid is identical in its qualitative composition with the serum of the blood. 2. *Fibrinous dropsy*, or that which contains dissolved fibrin, and in its chemical composition resembles the liquor sanguinis. 3. *False dropsy*, in which the fluid differs essentially in its chemical composition from either of the preceding forms.

Serous dropsy. Dropsy in which the effused fluid corresponds with the serum of the blood is by far the most common of any of the above forms. Most cases of ascites, hydrothorax, hydrocele, anasarca, and œdema belong to this division, as likewise do the fluids of pemphigus, blisters, &c. Vogel gives a good sketch of the chemistry of the fluids effused in the form of dropsy; as however this subject is fully discussed in the chapter "on the fluid products of disease," in Dr. Day's edition of Simon, we must refer our Sydenhamic—that is, we hope, *all* our—readers to that work for information on the point. Then comes a section on the *causes and modes of origin of serous dropsy*, in which it is traced to a "want of balance between the porosity of the venous walls and the specific gravity of the blood contained therein; that is to say, it occurs when the venous walls become more porous, or the blood lighter, or more aqueous than in the normal condition." In either case there is an increased transudation of serum through the walls of these vessels. This is the manner in which local dropsy invariably occurs where individual veins are compressed or in any way obstructed. It is thus that pressure of the impregnated uterus causes œdema of the feet and pressure on the *vena portæ* or *vena cava ascendens* produces ascites and œdema of the lower part of the body.

Although there is undoubted evidence that the blood under the influence of strong pressure gives origin to the effusion of dropsical fluids, yet we must not regard the process as a merely mechanical one.. Why does not the fibrin which is dissolved in the liquor sanguinis enter into the dropsical fluid? And why, as a general rule, is it that these effusions contain as large an amount of salts, and more water, but less albumen than the serum of the blood? The difficulty of answering these questions shows us that the process is more complicated than at first sight it seems to be. A more accurate acquaintance with the relations of endosmosis than we at present possess is requisite to explain these points in a satisfactory manner.

Fibrinous dropsy is regarded by our author as being of more frequent occurrence than the serous form, although it has seldom been described, and has never hitherto received a special name. In its chemical composition the effused fluid is identical with the liquor sanguinis; it is serum with dissolved fibrin. Vogel communicates seven analyses of fibrinous effusions, viz. one by Quevenne of the fluid of empyema, three by himself and two by Scherer of similar fluids, and one by Schwann of the fluid of ascites. Scherer's two analyses are recorded in p. 494 of the second volume of the English edition of Simon, which likewise contains analyses of similar fluids by Simon and Heller. In Schwann's case we cannot help thinking that there must have been some error in the chemical manipulation, the fibrin amounting to no less than 83 in 1000 parts of fluid, or rather more than twenty-four times as much fibrin as occurs in the liquor sanguinis. This fibrinous effusion is attributed by our author to the permeation of liquor sanguinis through the walls of the capillaries. In favour of this view he urges—1st. That the walls of the capillaries are much thinner and more delicate than those of the veins; and that the product of endosmosis will naturally be more concentrated and richer in solid constituents than in the latter case. 2d. That as serous dropsy is associated with dilatation of the veins and attenuation of their walls, so we learn from microscopic examination of the capillary system that a dilatation of those vessels and an attenuated condition of their walls precedes and is associated with the occurrence of the fibrinous fluid either in the parenchyma of an organ or in a cavity.

The fibrinous fluid is capable of organization, which is always effected at the expense of the fibrin contained in it. As far as development is concerned, it is indifferent whether the fibrin has coagulated within the body, or whether it has remained fluid, as in either case it acts equally well as a cytoblastema. There may be produced from it the most varied forms of tissue, either normal—as cellular tissue, simple muscular fibre, cartilage, bone, vessel, or nerve; or pathological—as pus, granular cells, cancer, tubercle, &c. Through this capacity for organization, fibrinous dropsy becomes the common source of a great variety of morbid growths.

False dropsies are made to include the cases of dropsy of the kidneys, gall-bladder, lachrymal sac, &c., which are common in the writings of the older pathologists. They are dependent on a closure of the duct of the secreting organ, and a consequent accumulation of the secretion.

PATHOLOGICAL RELATIONS OF THE BLOOD. Deviations in its physical characters, such as are directly obvious to the senses, are first considered; and, secondly, those of a chemical nature, which frequently require complicated processes for their detection.

Amongst the *physical changes*, the first in order is *change in colour.* After noticing the colour of the blood in cases of cyanosis and scurvy, our author remarks that he has "sometimes noticed venous blood of a bright red colour, with a shade of blue, much the tint that is developed on treating uric acid with nitric acid and ammonia. Thus, in the dissection of an arthritic subject, the blood of the renal veins presented this tint." The variations in colour are, however, not sufficiently constant to permit of our always finding the same tint in the same disease, as may be clearly seen by the subjoined table from the recent monograph of Popp* on the Blood in various Diseases:

---

* **Untersuchungen über die Beschaffenheit des menschlichen Blutes in verschiedenen Krankheiten, von Dr. Karl Popp, Leipzig, 1845.**

In 133 cases the colour of the blood was observed to deviate from the normal tint 41 times.

		Ordinary colour.	Very bright red.	Very dark brownish red.	Bluish red.
In 8 cases of	simple plethora	5	0	3	0
2	pregnancy	2	0	0	0
4	irritation of the brain and spinal cord	2	1	1	0
3	epilepsy	0	2	1	0
2	puerperal convulsions	0	2	0	0
2	hemiplegia	2	0	0	0
1	poisoning by lead	0	0	1	0
3	hypertrophy of the heart	3	0	0	0
3	simple dilatation of the heart	1	0	2	0
2	inflammation of the brain and spinal cord	2	0	0	0
1	inflammation of the brain	1	0	0	0
31	pneumonia	25	5	0	1
6	bronchitis	5	0	1	0
1	metritis	1	0	0	0
1	inflammation of the eye	1	0	0	0
1	erysipelas	1	0	0	0
12	febrile and acute rheumatism	8	3	1	0
3	simple rheumatic fever	3	0	0	0
10	typhus	2	3	0	5
2	scarlatina	1	0	1	0
24	phthisis	19	3	2	0
1	chlorosis, with dilatation of the heart	0	1	0	0
1	scirrhus	0	1	0	0
6	Bright's disease	5	0	1	0

The serum, which in the normal state is clear, is sometimes opaque and of a milk-white appearance. This may depend on various causes. Firstly, on a large number of microscopic fat-globules; secondly, on the presence of a considerable quantity of minute granules of coagulated fibrin; and, thirdly (very possibly), on the formation of a free acid in the blood, whereby a portion of the albuminate of soda becomes decomposed, and albumen, in a finely granular state, is separated. The turbidity dependent on the presence of fat is sometimes observed in perfectly healthy persons, shortly after partaking of an abundant meal, when the blood is receiving a large quantity of chyle. Turbidity, arising from granules of coagulated fibrin, has been observed by Scherer in a pregnant woman with *bronchitis tuberculosa*, in a leuco-phlegmatic person suffering from attacks of vertigo, and in a spirit-drinker with cerebral congestion; by Simon in a man with Bright's disease; and by Zimmerman in several morbid affections.

Deviations in the *consistence*, *mode of coagulating*, *odour*, and *taste* of the blood are then considered, and the section concludes with a few remarks on *changes in the corpuscles.* The author has carefully collected all that is known on these points, but has contributed nothing original.

We now arrive at one of the most important subjects in the whole work, and one to which we have already devoted considerable attention in this Journal (Vol. XVII, pp. 136 and 427, and Vol. XX, p. 76); namely, *deviations in the chemical composition of the blood.* Since the publication of Andral's two memoirs in 1840 and 1841, and the works on animal chemistry reviewed in Vol. XVII, the contributions to the chemistry of healthy and diseased blood have been almost overwhelming. Simon's Chemistry, which was published (at Berlin) in 1842, contained, in addition to thirty-two of his own analyses of diseased blood, a full account of all that was then known and of all that had been previously done on the subject.

The English editor has added to these the whole pith of the elaborate memoir of MM. Becquerel and Rodier, besides numerous recent analyses by Heller, Scherer, Rinskoff, Herberger, and others, and, in fact, has rendered the chapter on the blood a nearly perfect monograph on the subject up to the commencement of the present year. Although by no means so complete in its various bearings, Vogel's chapter on the pathological chemistry of the blood is still worthy of an attentive perusal by such of our readers as are familiar with the German language, although it contains little in the way of novelty requiring especial notice. His arrangement is excellent; and the following scheme will show that scarcely any important point in the chemistry of the blood is overlooked. The various morbid changes are divided into the two following groups:

I. Those in which any of the normal constituents of the blood are increased or diminished.

a An increase or diminution in the amount of fibrin.
b An increase or diminution in the amount of blood-corpuscles.
c An increase or diminution in the amount of water.
d An increase or diminution in the amount of albumen in the serum.
e An increase or diminution in the amount of salts.
f An increase in the amount of urea.

II. Those in which abnormal constituents are present in the blood.

a Free lactic acid.
b Carbonate of ammonia.
c A substance precipitable by acetic acid, resembling pyin.
d Sugar.
e Bile-pigment.

In addition to these, he mentions pus-corpuscles and entozoa as foreign organisms to be detected by the microscope rather than by chemical analysis.

In connexion with the determination of the blood-corpuscles by Figuier's method of analysis, which consists in retaining them on a filter after treating defibrinated blood with double its volume of a solution of sulphate of soda of spec. grav. 1·130, (see Day's Edition of Simon, vol. i, p. 190,) Vogel observes that he has always found the filtered solution of a somewhat red tint—a sign that the colouring matter of the blood is not perfectly insoluble in a solution of sulphate of soda. On the whole, however, he seems to approve of the process.

With regard to the abnormal constituents, free lactic acid has been found by Scherer in blood taken from the dead body in cases of puerperal fever and phlebitis, and on several occasions by Vogel, in the blood after death from miliary fever and rheumatism. We regret that no decisive proof of the true nature of the acid is given, as evidence for or against the occurrence of lactic acid in the animal body is just now of considerable importance. Enderlin, and indeed the whole Giessen school, deny that lactic acid can be formed in the human body; and we have the evidence of Pettinkofer that a zinc-salt may be obtained from the urine, resembling in all physical characters lactate of zinc, but differing in the circumstance of the acid containing a considerable amount of nitrogen. While on the other hand, Berzelius, Pelouze, Gobley, and Mulder as strenuously contend that, free or in combination, it occurs in almost every fluid of the body.

Carbonate of ammonia was found by Scherer in blood taken from the arm of a patient with typhus; and the substance resembling pyin was found by the same chemist in the blood of a woman who had died from metro-peritonitis. In his observations on the occurrence of bile-pigment in the blood, our author forgets to mention that the essential matter of bile (biliary resin, bilin, or choleic acid) has been detected by Orfila, Collard de Martigny, Clarion, Pettinkoffer, &c.

The remainder of the chapter is devoted to the consideration of *changes in the quantity of the blood*, *extravasation of blood*, *and the solution of the hæmatin and the consequent infiltration of the tissues.* These are all fully discussed and admirably illustrated by the Atlas.

We have now arrived at Chapter IV, which occupies no less than two hundred and eighty-three pages, and is devoted to the subject of morbid products and formations.

MORBID PRODUCTS. The following extract "on the development of morbid organic growths" will serve as a fair illustration of our author's plain, straightforward mode of handling his subject:

> "It is dependent on laws essentially different from those of mere chemistry. This difference even exhibits itself in the formative material—the cytoblastema; which is most commonly fluid, but may, however, be solid. If solid, it must of necessity be amorphous; in fact, the only solid blastema for morbid products which has yet been observed is coagulated fibrin in an amorphous condition and infiltrated with water, such, for instance, as occurs in inflammatory exudation. But even this blastema was originally fluid, and only became solid through the coagulation of the fibrin. No other coagulated protein-compound, neither albumen, casein, nor globulin, has ever yet been observed to act as a cytoblastema. In the formation of morbid products from a mother-liquid, a solid condition of the plasma has never been observed to occur.
>
> "As in the solid cytoblastema coagulated fibrin seems to act the leading part, so in the fluid cytoblastema dissolved fibrin appears of equally essential importance. This point is, however, of such importance as to require an accurate investigation. It is possible, in many cases, to isolate the fluid blastema of morbid products, and when it occurs in sufficient quantity to institute a chemical examination of it; as, for instance, in cases of exudation into serous cavities, or of a blister under the cuticle. In these cases we find it composed of water, fluid albumen and fibrin, fat, extractive matters, and various salts. That aqueous solutions of salts and extractive matters cannot of themselves alone act the part of a blastema to organized formations is beyond all dispute. They may, indeed, discharge the office of a mother-liquid; in many cases they may even enter into definite forms, as calcareous salts into bone, and chloride of sodium (according to Lehmann) into cartilage; but organized products can only be developed in them when a fibrinous fluid is also present, as, for instance, when exudation takes place into the uropoietic viscera or intestinal canal.
>
> "The same is the case with regard to the fats, some of which admit of deposition in a crystalline form (as cholesterin in gall-stones), and may likewise enter as constituents into organized formations, but none of which, either alone or associated with salts and extractive matters, can act as a cytoblastema; at least, up to the present time no certain case of the kind has ever been observed. There remain, then, as the essential constituents of the blastema only the above protein-compounds, which, however, never occur alone in the body, but always in com-

bination with the above-named substances. But not all even of these are capable of higher development. Fluids which, in addition to the other constituents, contain no other protein-compound than dissolved albumen, never appear to assume the functions of a cytoblastema. Common as albuminous dropsical effusions are, they never give rise to organized forms, unless they also contain fibrin. I have made a very large number of observations on this subject, and have never met with a single exception to the above law. Moreover, a fluid containing casein as its sole protein-compound, seems to be incapable of acting as a cytoblastema. In the milk, for instance, as long as it contains mere casein, we never observe pathological formations, for the so-called *corps granuleux* are dependent on the normal development of milk; but if fibrin becomes mixed with it, morbid formations, as, for instance, pus-corpuscles, are produced. On the other hand, in all fluids which we regard as blastemata for morbid products, fibrin has been invariably detected. Hence we must regard it as the necessary and, apparently, the only essential constituent of the blastema. This law, respecting the necessity of the presence of fibrin in these cases, which I have seen to hold good in several hundred instances, without a single exception, is not at all in accordance with the course of normal development. Thus the egg—the prototype of all formative fluids—contains no fibrin, its place being apparently supplied by albumen. There may probably be some connexion between this fact and the observation of Mulder, that the albumen of the egg contains one atom less of sulphur than the albumen of the blood, and that, consequently, in its ultimate composition it is identical with fibrin. In the nutrition of the perfect organism, the general nutrient fluid—the modified blood-plasma permeating the walls of the vessels—acts as the general cytoblastema for all new formations. Whether the fibrin is the only essential formative material, or whether the albumen likewise takes part in development, is a question which cannot be answered with the same degree of certainty as in the case of morbid products, since the normal fluid of nutrition can never be obtained free from extraneous constituents in sufficient quantity to admit of exact analysis." (pp. 79-81.)

This is succeeded by a sketch of the cellular theory which has been so frequently brought before the notice of our readers that, although Vogel has introduced a considerable amount of original matter, we shall pass on to the consideration of special morbid products. We must not, however, omit to mention, that the cell-theory is excellently illustrated in Plate 1 of the Atlas, which, in addition to a diagram of a cell on a large scale, contains figures of the cells of carcinoma, tubercle, pigment, &c.—in all, twenty figures.

Fluid Products. Pus. The first of the morbid products specially considered is *pus*, and we have no hesitation in stating, that in the thirty pages devoted to this subject every fact of importance to the pathologist will be found recorded. The physical characters of normal pus (*pus bonum et laudabile*) are briefly noticed, and we then arrive at the most perfect description of the pus-corpuscle that has ever yet fallen under our notice. After a description of its size, form, and general appearance, we come to the history of its structure, which we shall give at length:

"The corpuscles of normal pus are organized forms, for the most part of a cellular nature, with a nucleus, cell-wall, and contents.

"The cellular structure with a decided nucleus is only apparent in unchanged corpuscles when the cell-wall is very transparent and delicate. In the majority of cases the nucleus is covered by a granulated opaque cell-wall, and does not become visible till the latter is dissolved or rendered transparent by acetic acid.

"In other cases in which the development of the pus-corpuscle is imperfect, we see only the nucleus and no cell-wall.

"The *nucleus* does not lie in the centre of the pus-corpuscle, but is situated eccentrically, and is usually attached to the inner surface of the cell-wall. We may convince ourselves that this is the case by allowing pus-corpuscles to float and rotate under the microscope. It is only the larger nuclei that form an exception to this rule, for they are occasionally so large as to occupy the whole space of the pus-cell. The nucleus of the pus-corpuscle presents many peculiarities, and is so different from other nuclei as to require a somewhat careful consideration.

"In other cells, the nucleus is a simple body; but in the pus-corpuscle this is not always, nor indeed generally, the case. It is usually composed of from two to five granules, forming a composite nucleus. Sometimes, on treating fresh corpuscles with acetic acid or with a solution of common salt, we can observe a single nucleus indented like a trefoil leaf, or cloven into two, three, or four smaller nuclei. But it is not every nucleus that undergoes this change; in some cases it appears to resist the action of these reagents. The large corpuscles, with a diameter varying from the 100th to the 80th of a line, exhibit several (two, three, or four) such nuclei, each of which is composed of smaller bodies, insoluble in acetic acid.

"The corpuscles forming the nucleus, when, by the addition of acetic acid, they are clearly brought before us, present various forms; sometimes (generally in good pus) they are elliptic, and present an excavated cup-like form, resembling fresh blood-corpuscles; sometimes, however, they present a spherical or oval appearance. In some cases they are distinct from each other, even lying in different parts of the cell-cavity; but they are more frequently in apposition, and connected together.

"This composite character of the nucleus (as revealed to us by the action of acetic acid) is highly characteristic of normal pus. The only other cases in which it occurs are in young gland-cells, and in the most recent layers of pavement-epithelium; but here they are not so general as in pus. Hence it follows that the size of the nucleus is liable to great variations—from being entirely absent, it may fill the whole cell. Its usual limits are from the 800th to the 400th of a line. Nucleoli are but rarely found in the nuclei of pus-corpuscles.

"The *cell-wall* varies in thickness, and surrounds the nucleus more or less closely. In very delicate and young corpuscles it is extremely thin, smooth, membranous, and transparent; in older and certain peculiar sorts of pus, it is thick, tough, opaque, and studded with minute granules. In many cases the corpuscles have no distinct cell-wall, consisting merely of a nucleus and an irregular deposition around it, without any clearly defined outline, as may be shown, not merely by microscopic investigation, but by its relations towards endosmosis. This is especially the case with young, imperfectly-formed corpuscles.

"Moreover, there are many differences in the *contents of the cell.* In pus-corpuscles with a single nucleus, a well-marked membranous cell-wall, and consequently a cavity between them, we often find no solid body in the cell-cavity except the nucleus: the contents must therefore be fluid, and are doubtless identical with the serum of pus. In other cases, in addition to the nucleus, granular contents, with independent chemical relations, are observed. Sometimes the contents seem so thoroughly fused into the cell-wall, that the two form only a single substance—a solid, but soft mass, in which the nucleus is imbedded." (pp. 107-10.)

The endosmotic and chemical relations of the pus-corpuscle are then considered, and the individuality of the following parts of that organism establish:

1. The *substance of the capsule*, which is soluble in solutions of the caustic alkalies and their carbonates, of borax, and for the most part in saline solutions, as those of muriate of ammonia, nitrate of potash, &c.; in part also in acetic acid. It forms the wall and a portion of the contents of the cell, and is doubtless a protein-compound, very similar to and pro-

bably identical with that modification of albumen which is precipitable by water, and is again dissolved by the addition of neutral salts or acetic acid.

2. The *substance of the nucleus*, which is insoluble in acetic acid, swells in saline solutions, and dissolves in solutions of borax, of the caustic alkalies, and their carbonates. This likewise is a protein-compound, but its exact nature is doubtful.

3. The substance of which the *minute molecules* consist, which remain undissolved on treating pus-corpuscles with solutions of the caustic alkalies or of borax. Lehmann and Messerschmidt regard this as a protein-compound analogous to keratin (or horny matter,) and in many cases this seems to be true. Sometimes, however, these molecules consist of fat; they are then soluble in ether, and if the pus-corpuscles are boiled in that menstruum previously to the addition of the alkali, these molecules do not make their appearance.

Many kinds of normal pus contain nothing in suspension besides the regular corpuscles; others, on the contrary, contain minute roundish molecules, often in a very considerable quantity. They are always very minute, for the most part less than the 1000th of a line in diameter, and swim (either alone or in heaps) in the serum and between the pus-corpuscles, which are frequently studded with them. These molecules are sometimes protein-compounds,—probably abortive pus-corpuscles; in other cases they consist of fat.

Finally, there are sometimes infusoria present, minute *monades* and *vibriones*, especially in pus from foul ulcers. By feeding them with carmine, Vogel was sometimes able to bring into view the minute specks representing their stomachs.

Moreover, we sometimes find accidental ingredients, as for instance epithelium-cells, fragments of epidermis, crystals of cholesterin or of ammoniaco-magnesian phosphate, or flocculi of amorphous or semi-organized fibrinous exudations. These can be easily detected by the microscope.

In his observations on the serum of pus, the author seems to attach little importance to the substance described by Guterbock under the name of *pyin*, and regarded by that chemist as characteristic of pus. Vogel observes that it is much more commonly found in bad than in good pus; and further, that it occurs in other morbid products, as for instance in carcinoma. After several chemical analyses of pus, we arrive at the important subject of *its formation*, to the consideration of which nearly six pages are devoted. The two following points may be regarded as fully established: 1. Pus-corpuscles cannot be formed from albumen alone. 2. When the full development of the pus-corpuscles is completed, the fibrin of the plasma is consumed, and the remaining serum resembles the serum of the blood, or the fluid of serous dropsy.

Diagnosis of normal pus. The following remarks on this subject are well worthy of attention.

> "The recognition of this morbid fluid is apparently so easy that any one after once seeing it and observing the above physical properties would trust himself to distinguish whether or not a fluid were really pus; and yet there are numerous sources of deception. Fluids containing fragments of epithelium in suspension may

readily be mistaken for pus, and on examining the body after death we sometimes believe that we have discovered suppuration, when in fact no such morbid process has been going on. The examination of the fluid under the microscope is the best and, indeed, the only certain means of guarding against such deceptions. If that instrument reveals the presence of normal pus-corpuscles, and on the addition of acetic acid the characteristic nuclei appear, then we may be assured that we have been examining true pus. As I have repeatedly witnessed such deceptions, I will, by way of warning, mention two cases. A woman died from pleuritis with considerable purulent exudation into the pleural cavity. On examination I likewise found in the pelvis of the kidney and the ureter on each side a whitish yellow, thick, creamy fluid, which had all the physical characters of pus, and was mistaken for that fluid by the physicians who were present. As during life there were no symptoms of disease of the kidneys, and as dissection did not reveal any morbid change in those organs, the case was regarded as a demonstration of the resorption of pus, and of its subsequent removal by the kidneys. I examined this assumed pus microscopically, and found in it no trace of pus-corpuscles, but merely broken epithelium from the pelvis of the kidney and ureter.

"In another case, in which the patient died from peritonitis with exudation, the stomach and upper part of the intestinal canal were perfectly free from remnants of food and chyle, but contained a large quantity of a thick yellow fluid, which was mistaken for pus. In this case also the microscope showed that no pus-corpuscles were present, and that the fluid merely contained the broken cylindrical epithelium of the intestinal canal." (pp. 121-2.)

We fully agree with Vogel, that the microscope is of far more service in the detection of pus than all the so-termed pus-tests; further, it enables us not merely to distinguish pus from mucus, broken epithelium, blood-corpuscles, &c., but likewise to determine approximately the amount of those different substances, which chemical analysis has never succeeded in doing. The characters of Gluge's *compound inflammatory globule* are then noticed, and as we believe that very incorrect ideas are frequently maintained on this subject, and that too great importance is often attached to its occurrence, we shall devote a few lines to Vogel's description of it. Vogel objects to the term *inflammatory globule*, because its connexion with the process of inflammation is not more intimate than that of various other organized formations occurring in exudations; and further, because it is found under conditions in which there is very little probability of its resulting from inflammation—as for instance, in cysts in the thyroid gland. He prefers the term *granular cells*. When these granular cells occur in a perfect condition, they vary in diameter from the 200th to the 80th of a line; some are perfectly round, others oblong, irregular, and even angular. They appear to be agglomerations of minute granules, varying in diameter from the 800th to the 1000th of a line. The following is their mode of formation, as observed by Vogel in inflamed lungs. Cells with a nucleus and nucleolus, differing from pus-corpuscles in their larger size and in having a simple nucleus, are formed in the exudation. These become gradually filled with minute granules, which at first, when only few in number, readily admit of the nucleus being seen; subsequently, however, they conceal it, and the originally smooth cell-membrane becomes rugged, and the angular cell appears as a spherical agglomeration of molecules. Subsequently, the cell-wall appears to vanish, the inclosed granules separate and form irregular heaps, while each granular cell undergoes in miniature the identical process which a mass of coagulated fibrin undergoes in its conversion into pus-corpuscles. This is regarded by Vogel as their usual mode of development; he believes, however, that occasionally the process may be reversed,—that isolated elementary granules are first produced, which collect in groups, and are finally invested with a cell-membrane. Numerous varieties of pus-corpuscles and granular cells are delineated in Plate III. of the "Icones."

Solid products. The solid morbid products next claim our attention. Before proceeding to the consideration of the more strictly morbid textures we have a section extending over upwards of thirty pages, on the regeneration of normal tissues. There are successively considered the epigenesis of areolar tissue (*Bindegewebe*), of blood and vessels, of epithelium and epidermis, of fat and adipose tissue, of muscle, of elastic tissue, of granular pigment, of nervous tissue, and, finally, of cartilaginous and osseous tissues. The section is intended as an introduction to the important subject of *tumours*, which occupies the succeeding 128 pages.

In a histological point of view, tumours may be arranged in two great divisions. In the first, we place those whose histologic elements are identical with those of the normal body, and, further, which when once formed discharge the same duties as the other parts of the body, undergoing metamorphosis of tissue, and receiving their nourishment like a normal portion of the frame—*homologous*, *non-malignant tumours.*

In the second division we place those whose elements differ histologically from those of the normal body, and which from their nature give way, soften, and destroy the adjacent parts—*heterologous*, *malignant tumours.*

Non-malignant tumours are subdivided by Vogel into—

1. Tumours consisting principally of vessels—vascular tumours.

As a good account of these growths may be found in one of the recent volumes of the Sydenham Society (Swaine's edition of Hasse's "Pathological Anatomy," p. 102,) we proceed without comment to

2. Tumours consisting principally of fatty tissue—adipose tumours. A true fatty tumour may, according to Vogel, become converted

α. Into general infiltration of fat (*polysarcia*, *obesitas*) by local hypertrophy of the adipose tissue.

β. Into fibrous tumour by the addition of areolar tissue.

γ. Into encysted tumour by the formation of a cyst around it.

In relation to diagnosis, it must be remembered that many forms of malignant tumour (encephaloid) in their physical characters closely resemble fatty tumours, and can only be distinguished from them by microscopic examination.

3. Tumours consisting principally of fibrous tissue—fibrous tumours.

The following experiment throws some light on the origin of these tumours. Vogel injected several ounces of a solution of hydrosulphate of ammonia into the abdominal cavity of a large dog, and immediately closed up the orifice. At the expiration of twenty-four hours he was killed, and on examination there was found an amorphous exudation of coagulated fibrin on several parts of the intestinal canal under the peritoneum, and blood was extravasated between the muscular and serous coats. On the anterior surface of the stomach there was a coagulum of blood, as large as a hazel-nut, surrounded by a thick layer of fibrin, and firmly attached to the outer wall of the stomach. "I am firmly convinced," adds Vogel, "that this coagulum would in time have been converted into a fibrous tumour, if the animal had not been killed. I have had frequent opportunities of making similar observations. This appears to illustrate the probable origin of fibrous tumours in man—at least of such as occur in the stomach, the intestinal canal, and more especially in the uterus, where there are more frequent opportunities for the formation of coagula."

4. Tumours in which cartilage predominates—cartilaginous tumours.

5. Tumours in which osseous tissue predominates—osseous tumours.

6. Tumours consisting in whole or in part of dark pigment—melanotic tumours. The pigment entering into these tumours is by no means uniform in its characters. In many cases it consists of dark granules inclosed in round or oval cells, sometimes it is modified hæmatin, and it is occasionally composed of granules of sulphuret of iron. The first constitutes true, the two last, false or spurious, melanosis. The pigment is never the sole constituent of these tumours, the other elements being fibrous tissue, a few vessels, and not unfrequently malignant growths, as tubercle, encephaloid, or scirrhus. In true melanosis the colour is brown (of a bistre tint), blackish, or if only a little pigment is present, gray; in the false variety resulting from altered hæmatin, it is blue, violet, or brownish-black; and in that depending on sulphuret of iron it is of a slate-gray, or greenish-black. Melanotic tumours are more frequently observed in the female than the male sex.

7. Tumours containing gelatinous matter—gelatinous tumours; which are described by Vogel as follows:

> "In many tumours there occurs a viscid jelly-like substance, partly infiltrated amongst the firm elementary tissues, and partly contained in appropriate spaces or cavities, sometimes in such abundance, and so greatly exceeding the other elements, that the tumours may with great propriety be termed gelatinous. The elements coexisting with gelatine in these tumours are various, generally fibres, vessels, and occasionally cartilage, and most commonly of all, cancer-cells forming gelatinous cancer—*colloid.*
>
> "This substance is always transparent and colourless, sometimes fluid, resembling thickened mucus; at other times resembling half-fluid jelly. Under the microscope it appears completely amorphous, and so perfectly transparent that it is not easy to see it. In six cases in which I have examined it, its physical and chemical characters appear constant and identical with those of mucus. ....Nothing can as yet be stated with certainty respecting the origin of this substance; it arises, however, in all probability, like normal mucus, from modified protein-compounds of the blood." (pp. 204-5.)

8. Tumours inclosed in a true cyst—encysted tumours.

The *malignant tumours* are divided into two leading groups. The former embraces those in which little (or no) organization is apparent, namely:

1. *Typhous deposits.*
2. *Scrofulous deposits.*
3. *Tubercle.*

The second includes the more highly organized secondary formations; namely, *carcinoma,* subdivided into

1. Cellular cancer—encephaloid.
2. Fibrous cancer—scirrhus.
3. Melanotic cancer.
4. Gelatinous cancer—colloid.

We pass over these subjects without comment; the character of the typhous deposit having been noticed in our review of Rokitansky, in Number XXIX, and cancer fully discussed in our recent review of Professor Walshe's 'Treatise.'

The remainder of this lengthy chapter is devoted to the consideration of unorganized morbid products.

The following scheme includes the various substances capable of occurring as deposits in the human body:

1. Protein-compounds.
2. Fats: *a*, cholesterin; *b*, margarin and margaric acid; *c*, olein; *d*, fatty granules of uncertain composition.
3. Uric acid and urates.
4. Salts of lime: *a*, oxalate of lime; *b*, basic phosphate of lime; *c*, carbonate of lime.
5. Ammoniaco-magnesian phosphate.
6. Sulphuret of iron.
7. Bile-pigment.
8. Silica.
9. Various substances of easy solubility, deposited in consequence of evaporation, &c., as chloride of sodium, phosphate of soda, &c.

From one or more of these constituents are formed all the concretions occurring in the human body. These are arranged by Vogel into

1. Such as are produced from the secreted fluids.
2. Such as are formed in the parenchyma of organs.

The subject is completely treated in all its bearings, but contains little with any claim to novelty. The analyses of the concretions are for the most part taken from Simon.

We now arrive at a short chapter of twenty pages on the morbid changes in the physical properties of the tissues and organs. Changes of colour, volume, form, and consistence, are discussed, and a considerable amount of original matter introduced.

After a very brief and somewhat satisfactory chapter on the combinations of morbid changes, we come to the consideration of *independent organisms in the human body—parasites.* These are divided into *Epiphytes* and *Parasitic animals.*

The *Epiphytes* are subdivided into (1) vegetations in the human fluids, namely, the *torula cerevisiæ* in vomited fluids and fæcal evacuations, and Goodsir's *sarcina ventriculi;* (2) vegetations on the external skin and its appendages, namely, in *Tinea favosa* (Gruby, Fuchs, Bennett), in the sheath of the hair in *mentagra* (Gruby), within the root of the hair in *Herpes tonsurans* (Gruby, Hebra), and in *Plica polonica* (Gunsburg); and (3) vegetations on the mucous membranes, as, for instance, in the aphthæ of children, in the cicatrices of the mucous membrane after typhus, &c.

Parasitic animals are subdivided into infusoria, insects, arachnida, and worms.

The following experiment, in relation to infusoria in the blood, is interesting, although leading to a negative result. Two ounces of water, containing millions of infusoria of the same species (either *monades* or the young of the *cyclidium glaucoma*), were injected into the vein of an adult cat. At the expiration of twenty-three hours not a trace of the infusoria could be observed, and when the animal was killed, two days subsequently, the search was equally unsuccessful. If Siebold's* article on parasites is studied conjointly with the present chapter of Vogel, the reader will acquire an immense amount of information that may in vain be sought for in English or French medical literature.

Congenital changes in the human body are then considered. The causes of malformation are, according to our author, the following:

1. Abnormality of the generative material of one or both parents.
2. Abnormality of the maternal organism, pathological changes in the uterus, fallopian tubes, &c.
3. Diseases and abnormal conditions of the placenta, membranes, or cord.
4. Pathological affections directly affecting the fœtus—diseases and mechanical injuries.

The different forms of malformations are arranged in eight classes, most of which are divided into orders and genera. The difficulty of condensing this subject within the limits of some forty pages will be at once recognized by those who are conversant with the voluminous works of Meckel, Geoffroy St. Hilaire, Otto, Gurlt, Vrolic, &c. The present chapter (although little more than a syllabus) contains infinitely more available matter than the works of any one of the authors just quoted.

The part now before us concludes with a chapter on the changes occurring directly after death. Like all that precedes it, it is well worthy of perusal, but contains no especial novelty either in the way of arrangement or material.

From our previous knowledge of the writings of Vogel, and from the high reputation that he has acquired during the last seven or eight years (and pathological histology is of no older birth), we took up our pen with a decided prepossession in his favour. If we have been slightly disappointed in not finding so many original observations as we expected, that disappointment has been more than counterbalanced by the philosophic spirit pervading the whole work. Unlike Lebert, whose work is noticed in our last Number, he strives not for glory but for truth, and every page shows his desire to render to all men their due. Here we have none of the self-gratulation and assumed superiority for which we were compelled to inflict a moderate castigation on the pseudo-Frenchman; on the contrary, we can lay our hand on several pages in which M. Vogel desires to recant or modify opinions he had previously adopted; and the truthful singleness of mind he displays cannot fail to have its due weight with his readers. In his knowledge of the foreign literature of his subject, we may express our decided conviction that he stands unrivalled. Carswell and Hope, Owen and Liston, Simpson and J. H. Bennett, are but a few of the British names that meet us in his pages. France, Italy, Holland, and Denmark,—all lay open their stores before him, and he gathers as freely from Cruveilhier and St. Hilaire, Cerutti and Bassi, Vrolik and Broers, Hannover and Steenstrup, as from his own countrymen Müller, and Henle, and Gluge.

We have had occasion, in several parts of this review, to notice the

---

* Wagner's Handwörterbuch, &c., vol. ii, pp. 641-92.

Atlas; but if we spoke of it as a mere volume of illustrative plates we should be doing its author a great injustice. Of the 291 figures contained in it, 270 are drawn from nature, and accompanied with copious explanations extending over 128 quarto pages. Of the accuracy of most of the figures we are enabled to speak from experience.

We are glad to learn that this work is likely soon to appear in an English dress, from the competent pen to which we are indebted for the excellent work of Simon, recently issued by the Sydenham Society. We trust that Dr. Day will incorporate in it the most important cases from the 'Icones,' and have it properly illustrated. We do not doubt that a work of this nature, with microscopic delineations of morbid products, would supply a want which has been long experienced in this country.

Art. V.

Geschichte der Findlinge in Oesterreich mit besonderer Rücksicht auf ihre Verhältnisse in Illyrien. Von Dr. Raimund Melzer, K. k. Director der Staats- und Local-Wohlthätigkeits-Anstalten zu Laibach.—*Leipsig*, 1846.

History of Foundlings in Austria, with especial reference to their Condition in Illyria. By Dr. Raimund Melzer, Imperial Director of the State and Local Charitable Institutions at Laybach.—*Leipsic*, 1846. 8vo.

The author of this able and learned work is unusually well qualified for the task he has set himself. Trained in the service as well as in the superintendence of the commission for inquiring into the state of the poorhouses and houses of correction, then acting in the capacity of professor of midwifery and physician to the lying-in and foundling hospitals, and, lastly appointed to the responsible post of director to the state and local charitable institutions, he has been diligently employed, during ten years, in the performance of duties which have familiarized him with the causes and effects of poverty. It was natural that the institutions with which he was most intimately connected, first as physician, and subsequently as director, should engage the greater share of his attention. Accordingly, it scarcely required the stimulus of the example of the prize essays of MM. Terme and Monfalcon, of which the reader will find elaborate notice in the Twenty-sixth Number of our Review, to urge him to the laborious inquiries which have issued in the work before us—a work for which his habitual occupations, and his access to public documents, peculiarly fitted him.

His mental qualifications for his task will appear of no mean order to any one who will take the pains to peruse his Introduction, in which he shows that he has not been a careless observer of poverty and the poor, and that the great and perplexing alternations which have beset all national efforts for the relief of indigence have not escaped him. These alternations have rarely been better expressed than in the following passage:

> "Poverty has the great misfortune that it borders closely on vice, and easily passes into it. Nay, we may call vice a moral poverty. Two steps further, and

the pauper becomes the criminal. If charity is not circumspect, it will be deceived and cheated by vice or crime, and innocent poverty will be robbed of the assistance which was intended for it. How easily this may happen, may be inferred from the familiar fact that natural benevolence is easy of belief, and from the mistakes which philanthropic persons are continually making. As often as poverty is mixed up with vice it is in a state of resistance against law, for vice is in its very nature lawless. This circumstance is the source of many evils with which even the best administration of Foundling Hospitals has to contend, and is the reason why several governments exclude those establishments from the lists of charitable institutions, and esteem them a greater evil than the existence of those whom they are intended to assist."

Our author shall state in his own words the case of foundling hospitals, and shall exhibit the way in which the charity which presided at their foundation has been and is deceived and cheated:

"The Christian pity," he says, "which called these institutions into being, went so far in its anxious care for mother and child as in their reception to make itself blind. These institutions had not existed long before they became the dupes of vice. They were established to preserve purity of morals, and they soon began to undermine them. The stricter the principle of secrecy was preserved, and the further it was carried, the more shamelessly was it abused. Parents, bound by the ties of marriage, forgot their most sacred duties, and relieved themselves of children whom they regarded as heavy burdens; and they did this because the regulations of the institution tempted them to it—because they saw themselves thereby exculpated from a punishable offence, and even justified in their unnatural conduct. The foundling hospitals were crowded with applicants, and the expense increased with their increasing numbers till the evil reached an alarming height. To prevent this monstrous outlay from increasing, and, if possible, to effect a permanent diminution of expense, without endangering the original humane aim of the institution, formed the problem; the solution of which became the anxious wish of the government and of every friend of humanity."

And is not this the history of all charities without exception, and especially of all so-called national charities? Let us suppose a large town, if such a supposition be reasonable, in which no such thing as a beggar is to be found, but it is known that a large number of its inhabitants are poor, many in want, and some few, perchance, starving. A benevolent person, aware of these facts, proclaims them, and appeals to his fellow-citizens in behalf of the sufferers, insisting that they ought to carry purses in their hands, and invite the destitute to apply to them for alms as they walk the streets. What would be the consequence? Why, what other result could follow but that which has followed from the habit of dropping halfpence into the hands of street beggars—the creation of a band of masqueraders, who make mendicancy their profession, and come to think it to the full as honest as working with their hands, and somewhat safer than stealing, but who are much more likely to forget the danger of petty larceny than to be reminded of the safety and respectability of honest labour. And what is the total result of this misguided charity? This: to draw off, by the constant dropping of halfpence and pence into idle and unworthy hands, the fund which was destined to the support of honest labour, and which, so applied, would have rescued a greater number of deserving people from want, and saved more victims of threatened starvation than all the idle almsgiving in the world. It would probably have prevented, and most certainly have contributed to prevent, that very distress which brought the idle habit of street-almsgiving into existence.

At length the public, though slow to learn, discovers the error into which it has been led, and street-begging begins to be discouraged. Naked feet, bandages, and tattered fancy dresses are at a discount, the Mendicity Society is in favour, and charity takes a new direction. She has been imposed upon in the street; she will try the houses. There, at least, she must be safe. She can inquire into the particulars of every case of destitution and suffering, and her purse-strings shall not be drawn till she has satisfied herself that her eyes are not deceived, nor her heart melted by the trick of the theatre. Poor dupe? Broken chairs, tattered and scanty bedclothes, and all the marks of domestic poverty and wretchedness, have replaced the rags and bandages of the street, and the affected piety of the home hypocrite and masquerader, the whine of the street-beggar. And here, too, the funds which might have ministered to the support of honest industry, and the relief of real want, are diverted from their proper channel by the treacherous arts of the house-beggar. Take one other case. This nation piques itself upon its poor-law, and on its generous determination that no man in the whole length and breadth of England shall be allowed to starve. Good. Britannia looks well in the attire of my Lady Bountiful. But is she quite sure that she does more substantial good by her annual expenditure of millions—that is to say, more good in proportion to the outlay—than the patron of street-beggars or home-impostors? Is out-door relief more free from imposition than street-almsgiving, or the union system much more free from abuse than the district visiting societies? Are not the millions expended every year as certainly drawn from the fund which would support honest industry and *prevent* destitution? and is the total good effected in any degree proportionate to the total expenditure? A fact was recently mentioned in the course of a debate in the House of Commons, which, strange to say, seems to have passed unnoticed by those who have the eye of a lynx for any abuse of the poor-laws, provided it be only traceable to the faults of their administrators. As many as four thousand pensioners were distinctly stated to have squandered their pensions in drink, and then to have entered the workhouse. Of course they did. What better could be expected of a certain considerable portion of society than that they should avail themselves of the houses so kindly opened for the encouragement of idleness, intemperance, and profligacy. Then what an instrument of rural tyranny that same workhouse system is! How convenient the threat that if wages, brought to the level of starvation, be not accepted, there is the workhouse hard at hand, where the honest labourer shall be introduced to all the idleness and profligacy of his neighbourhood, and supported (cruel mockery!) by the very funds which ought to have contributed to pay him a fair day's wages for a fair day's work.

We have been provoked to say more than our readers might expect to find in a Medical Review on this great question of so-called charity, individual and national, and to throw out these evidences of a scepticism as to the real good effected by its means, by our author's unjust censure of a man who, whatever his faults as a reasoner, cannot justly be accused of any want of humanity, in spite of the severe and, in some respects, well-merited strictures of Sadler,—we mean Malthus.

The theory of Malthus was, doubtless, a false theory, and his fears of an increasing population idle and visionary; but his opposition to the poor-laws, and his assertion that no man has an abstract right to support, whether he will or will not work, may be, in reality, in its necessary results, far more humane than the easy and good-natured theory which it opposes. The question really is, whether the funds devoted to charity effect more in the way of cure than these same funds, employed in the support of labour, would accomplish in the way of prevention. Malthus may have been wrong in upholding the latter alternative; but it does not follow that his error, if it be one, was the offspring of an unfeeling disposition.

With this short protest against our author's disrespectful notice of the tenets of Malthus, and without stopping to qualify our own broad strictures on the evils of many of our modern charitable institutions, we proceed to a more intimate examination of the question of Foundling Hospitals. Considered in the light of charities, they are peculiarly open to the abuses which cling to all ill-considered plans for the relief of suffering and destitution; and it happens, fortunately, that the shrewd sense and right feeling of Englishmen have, very generally, condemned them; though with the narrow and exclusive views which, unfortunately, characterize highly practical nations as well as highly practical individuals, the merited censure they have received has not been extended to other so-called charities open to similar, if not greater abuses. At first sight it would seem an unmixed good, as well as an obvious Christian duty to succour and support such children as may be found abandoned by their unnatural parents; but this duty must have some qualifications, and this good some limits. What virtue or what blessing has not? Where, then, are these qualifications and limitations to be found? The answer is easy: when the practice of a duty, or what appears to be such, entails more evil and more suffering than it relieves, it is obvious that we have been acting under a mistake, and that we must abandon or modify our course of procedure. The point at which the evil predominates over the good is evidently the limit which we ought not to pass beyond. In the case under consideration, the object we have in view is the preservation of human life.

This just and sacred object we, undoubtedly, accomplish so long as in our reception of the foundling we hold out no encouragement to illicit intercourse, or the multiplication of deserted children. But no sooner do the measures which we adopt, whether as individuals or communities, act as an advertisement of our charitable intentions and actions, than deserted children begin to increase till, at length, they are equal to the utmost demand which our philanthropy has created. Let us take the extreme case, that children of the tenderest age are received at all hours of the day and night, without question or limitation, as was the case, till recently, in France, under the liberal system of *tours;* and it follows, inevitably, that we are the indirect means of destroying more lives than would have been lost, had every foundling been suffered to perish in the streets. For we contrive, at one and the same time, to hold out an encouragement to illicit intercourse, and to the barbarous desertion of legitimate children; and by bringing so many children together under circumstances so unfavorable to infant life, we are the instruments of a more wholesale destruction of life than could have happened, had the comparatively few deserted children been left to the casual care of individual philanthropists, or to the fate to which their unnatural parents had abandoned them.

The suppression of the *tours* was, doubtless, a great improvement on the old system; but even with this improvement it admits of grave doubt, whether the encouragement held out to illicit intercourse, on the one hand, and the mortality incident to the best regulated foundling hospitals, on the other, do not lead to a larger number of deaths than would have happened had the matter been left to take its own course.

But we have entered quite as much at length into this subject as our limits will allow; we are bound to proceed to the further notice of the historical and statistical details prepared for us by the industry and research of our author.

The history of the establishment of foundling hospitals in Austria closely resembles that of France. A citizen of Laybach, one Peter Bellach, in the year 1041, devoted the whole of his property to the establishment of an orphan-institution, which was also to receive foundlings, and to train them to a trade. The example of Laybach was soon followed by other towns, and, at length, in Austria as in France, the foundling hospitals fell under the superintendence of government.

The history of legislation on this important subject forms the subject-matter of 170 pages, which we pass over, as it scarcely admits of being rendered interesting to the medical reader; all that can be of any importance to him will be found in the following notice of the statistics of the subject, which occupies the remainder of the volume.

The number of foundlings, in the several foundling hospitals of the Austrian dominions, has increased, during the twenty years from 1820 to 1840 in an arithmetical progression, and has doubled within that period. The average yearly number amounted to 49,317. In the year 1839 the population of Austria, exclusive of Hungary and its dependencies, was 23,389,959; the number of foundlings 59,026, being 1 in 396. The total in the twenty years amounted to 986,345, or little short of one million! In the years 1833–40, the total number of new-comers amounted to 128,658, or an average of 16,082. The numbers received in 1840, bore to those received in 1833 the proportion of 116 to 100; Gallicia is excluded from this calculation.

In Laybach the increase of foundlings has been very considerable. In the year 1763 one foundling was admitted; in 1842, eighty years later, the number amounted to 201; in the first twenty years there were 77 admissions, in the second twenty years 962, in the third 1439, and in the fourth 3275. The increase of females received into the lying-in institution in the same town was also considerable and progressive, though not so great as in the case of foundlings; the out-patients fluctuated, but showed, on the whole, a tendency to increase.

What is the cause of the increasing number of foundlings? is a question of considerable interest to which our author devotes a short chapter. The number of foundlings, as we might anticipate, is chiefly recruited from illegitimate births, which, as a general rule, increase with advancing wealth and civilization. In accordance with this acknowledged principle, it is found that the number of foundlings is least in the poorest districts; poverty, therefore, is not the cause of the abandonment of children. This position is abundantly proved by an appeal to facts. The most prosperous years are shown to be those in which the number of foundlings is greatest, while years of scarcity and war have often been marked by a decrease in number. Returning prosperity, also, is marked by an increase of foundlings, and the periods of the year in which there is the greatest distress, the winter months, are characterized by no marked influx of deserted children: thus, in Laybach, the number of foundlings in July and February was within an unit of the same. In Krain the number of deserted children was greatest in the month of May; March and April came next in order, then August. In the foundling hospital of Vienna, the months stood in respect of births, in the following order, beginning with that in which the number of foundlings was the greatest,—February, March, May, January, April, October, December, September, June, November, August, July; but the greater number of foundlings were, at Vienna also, received in summer.

It would appear then, that vice is more influential in increasing the number of foundlings than poverty, and that this species of vice at least increases with wealth and civilization.

The next question which Dr. Melzer examines is the proportion of foundlings born in wedlock and deserted by their parents. We were scarcely prepared for so startling a result as that which he has deducted from the records of the foundling hospitals of some of the Italian cities. During the period of ten years, from 1830 to 1840, there were received into the *tours* of the hospital at Milan 26,147 foundlings, of which 6,610, or 25 per cent., were legitimate; in Vienna, for the same period, there were only 111 in 3424, and in Brescia, 1744 in 20,148. In Protestant countries where there are no foundling hospitals and no *tours*, these children would not have been deserted by their parents; the Roman Catholic system, as our author observes, must, therefore, bear the blame of this increase of foundlings.

Another abuse of the foundling system is the abandonment of children by mothers, who offer themselves, immediately after, as nurses, in the hope of being paid for tending their own children; this abuse was pointed out, as occurring in the French hospitals, in the review to which we have already referred.

The mortality in childbed and that of foundlings next engages the attention of our author. The mortality in childbed at the lying-in institution at Laybach, has been very low compared with that of many other similar establishments in Austria. While it has been 6 per cent. in Vienna, 5 in Milan, 3 in Lemburg, 2 in Prague, Linz, and Venice, it was less than 1 per cent. in the Tyrol, and 80 in the 1000 at Laybach. This high mortality of Vienna is attributed, and with apparent justice, to overcrowding; and our author strongly recommends increased accommodation, quoting, for this purpose, the dictum of Dr. Ferguson, "a lying-in hospital should consist either of a series of cottages, or its spacious wards should contain very few patients."

The mortality of children is very minutely examined, beginning with still-born children; it appears that in the several lying-in institutions of the Austrian Monarchy, they bear to those born alive the following proportions;—Gallicia, 1 in 7; Dalmatia, 1 in 11; Lombardy, 1 in 12; Maritime districts, 1 in 13; Venice, 1 in 16; Upper Austria, 1 in 18; Lower Austria, 1 in 25; Steiermark, 1 in 26; Tyrol, 1 in 27; Bohemia 1 in 28; Moravian Silesia, 1 in 38; Carinthia, 1 in 129. The average is 1 in 24; the proportion of still-born males to females was 124:100. The autumn months were found to be the most fatal, then the winter and spring, and the summer least fatal; but to this general rule Vienna forms an exception, the greatest mortality occurring in summer. In the Laybach institution premature births occurred in the proportion of 1 in 44 mature births; of these premature births, the greatest number occurred in the eighth month. Out of 91 premature births 5 (3 boys and 2 girls) survived, and 86 were born dead. The still-births, were, therefore, to those born alive, as 17 to 1; and the proportion of males to females as 139 to 100. The proportion of twin-births, in one of the lying-in institutions, during a period of fifty-one years, was 71 in 4669, or 1 in 66. In France the proportion is 1 in 95; in Germany, 1 in 80; in England, 1 in 92; in Scotland, 1 in 95; in Ireland, 1 in 62. In these twin-births there was one male to three females; 17 twins in the 100 were born dead, and where one was born alive and the other dead, the number of still-born males was, to that of still-born females, as 1 to 2⅓. The accoucheur will find some interesting details of the relative frequency of natural and unnatural presentations, to which we refer him, while we proceed to the mortality of the inmates of foundling institutions. In a period of fifty-three years there was born in Krain, on an average, 102 foundlings, of whom, 67 attained the age of ten years, and 33 died before that age. In the first twelve years of life the deaths numbered 34½ per cent.; the mortality of the first year was 23 per cent., which fell short of the mortality in the community at large, which amounted to nearly one-third. The mortality of male children was to that of females as 6 to 5; but it must be borne in mind that the male children exceeded the females by 5 per cent. The mortality of males, therefore, supposing the numbers of the two sexes to be equal, evidently exceeds that of females.

If we divide the fifty-three years from 1789 to 1841 into two parts, it will be found that the mortality of the first half is less than that of the second; the cause of this difference is not stated; it would probably be found to depend on increasing numbers, without a corresponding increase of accommodation.

The mortality of foundlings in Vienna is minutely examined by the aid of figures. During twenty years (from 1821 to 1841) the mortality ranged from 12 to 23 per cent. In the first ten years the average mortality was lower than in the last, and in the three years, 1839-41, it was much greater than in the three years, 1821-23; the mortality of foundlings in Vienna has, therefore, increased. It appears that in different parts of the Austrian Empire there is considerable difference in the mortality of foundlings; thus, while in Lower Austria it is 18 per cent., in Bohemia 15, and in Steiermark 11, it is only 6 per cent. in Upper Austria and Illyria. This is the mortality of those brought up out of the foundling hospitals.

The mortality of foundlings is still further investigated for all parts of the Austrian dominions. This, as it would interest a small portion only of our readers, we shall pass over, and merely notice such facts as are of general interest.

It has been stated that the mortality of foundlings in Vienna has increased of late years; this is the case also in many other parts of the Austrian dominions, while in about an equal number of districts it has fallen off. The cause of this difference is not stated.

In illustration of the danger attending the exposure of the new-born infant to cold, some interesting facts are stated on the authority of Trevisan. Of 100 children born in Italy during the winter, 66 died during the first month, and only 19 survived their first year; while, on the other hand, of 100 children born during the summer, 83,—of 100 born during the spring, 48—and of 100 born during the autumn, 58 survived the first year.

Dr. Melzer's work abounds in statistical details, which want of space prevents us from noticing at greater length; we must, therefore, hasten to the concluding chapter of the work, in which the real value of the foundling-hospital system is tested by argument and fact. We have already touched upon this subject, but its importance demands a further notice.

The foundling system is differently ordered in Roman Catholic and Protestant countries. Though the object in both is the same—the preservation of the life of the innocent and helpless child, and the support of public morality,—they aim at accomplishing the object in opposite ways. The Roman Catholic system establishes lying-in and foundling hospitals, forbids any inquiry into the paternity of the foundlings, and loads the state with a heavy expense. The reception of the foundling is either by the *tours*, in which the concealment of the birth is the leading object—this is the Italian system; or the child is received *à bureau ouvert*—this is the French system. These are the two chief varieties of the Roman Catholic system. The Protestant system, on the other hand, pronounces lying-in and foundling hospitals to be an evil, and rejects them altogether; disapproves concealment, makes the mother answerable for the care of her child, and fines the father in the cost of its support. Where this system prevails illegitimate children are even more numerous than in Roman Catholic countries;* but children are very rarely abandoned. In a financial point of view, of course, this system has a great advantage over that of Roman Catholic countries.

It is impossible that these two systems can be equally valuable; the one must be better than the other; nevertheless, though different in their principles, they show a certain reciprocity in their operation. In the ultimate results of each system we must seek for the proofs of its utility. We must weigh the profit of each system against the moral and pecuniary results, to determine which has the advantage over its rival; but in actual practice we do not find such a relation between the principle and its results as we might have anticipated. To exhibit the real state of the case is the object of political economy; but, in the mean time, let us compare the argument for and against each of these rival systems.

---

* The proportion of illegitimate to legitimate births in England is 1 in 12; in France, 1 in 13; in Saxony, 1 in 7; in Würtemberg, 1 in 7-8; in Hesse, 1 in 5-6; in Prussia and Sweden, 1 in 13. (Springer's Austrian Statistics.)

The supporters of the Roman Catholic system shall have the first word. According to them foundling hospitals were among the first-fruits of the divine principle of the love of one's neighbour. Religion, not powerful enough to banish vice, and fearing to stain herself with the blood of the innocent, threw over it the veil of compassion, and led back the unfortunate offender to the bosom of society, out of which the hand of her parents had thrust her. Grief, shame, privation, and despair of being able to preserve the life of her offspring, offer a strong temptation to desertion, child murder, and abortion. To force the father to marry the woman he has betrayed, would be a doubtful good, to oblige him to support his child, is a certain difficulty; but in the asylums which are open to the mother, and in which she finds every needful assistance, she is safely and secretly delivered of her burden, and returns in peace to the world; she escapes the shame and reproval which otherwise await her, conceals her first offence, and can resolve against its repetition. The foundling hospital completes what the lying-in institution had begun; it relieves her from a heavy burden, and prevents her from sinking to a lower point of degradation. The child, whom the mother has consigned to the care of the foundling hospital, is not merely supported, but educated. How many mothers, a prey to shame and destitution, and sorely tempted to the commission of crime, has this system saved from destruction! how many children has it rescued from death* and from the fatal influence of bad example, and preserved to the state as moral and industrious citizens!

If we now turn to the Protestants, they admit the good intentions which led to the establishment of lying-in and foundling hospitals; but they look to their results, and inquire how far the sanguine expectations of their founders have been sealed by experience. These institutions were established to afford the poor destitute mother and child the assistance they so much require; but, however fine in theory, experience says little in their favour. Where such institutions exist, we are constantly meeting with the most melancholy results of such experiments,—men separated from their parents, strangers in their land, and brought up by hireling nurses and attendants. What might once have been a necessity, has long ceased to be such. The public opinion has come to pass a milder sentence on unchastity. By the tenderness which the law has shown to the shame of the mother, shame is at length banished, and with it all fear of consequences. If her poverty be assigned as a reason for our interference, the question arises, is the state bound to undertake the support of all the poor without exception? By doing this, it would step into the place of individual charity; it would give to poverty that which diligence and virtue ought to give it; and, by so doing, would encourage a mean spirit of dependence. The poor ought, it is true, to be supported, but the support offered by the lying-in and foundling hospitals is useless, at the same time that wisdom requires and offers better means. Among the poorer classes there are many married couples who look with envy on the illegitimate child and on her offspring, and on the liberal assistance and support which they have received in the lying-in and foundling hospitals. The child is supported at the public expense, and the mother admitted as wet-nurse into some rich or distinguished family. The conviction that, on the whole, the condition of the unmarried female is more tolerable than her own, that her own children can have no better care than that which the foundlings receive, is not far removed from the wish to turn those public institutions to her own use. This wish soon ripens into act, where the institution gives facilities for it. Hence the multitude of legitimate children who are taken out of the *tours*, and which increases as the fact becomes notorious. The mother has taken her legitimate child to the *tour*, the next step is to offer herself, after a few days, at the institution as wet-nurse; when she has the chance of receiving back her own child, and being paid for its support. To what abuses such a system may lead it is unnecessary to point out; the abuses of the system extend until large numbers of females, in a condition to support their own children, avail themselves of these public institutions. To return to the case of the single woman,—the support of her child, which, in the absence of these institutions, must have devolved upon her, might perhaps have saved her from a repetition of her fault; but the relief from these cares tempt her back to her former mode of life, and encourages her in her vicious course; and yet we hear of the moral uses of the foundling hospital, and of its favorable influence on public morals. It is asserted that lying-in and foundling hospitals tend to preserve the lives of children, and to prevent the exposure and murder of the new-born child. Experience, however, has shown how unfounded this statement is; it has proved that there are more exposures and infanticides in countries which have lying-in and foundling hospitals, than in those which are without them; and the same difference is found to obtain in the same countries before and after the establishment of their hospitals. Thus, in Krain, before the opening of the foundling hospital, the annual exposures amounted to $3\frac{15}{21}$ per cent.; after the opening of the institution $4\frac{1}{5}$ per cent. That criminal abortions and infanticides are not diminished by foundling hospitals, is also proved by tables to which we are obliged to refer the reader; neither is the assertion that foundling hospitals preserve the lives of children better founded. Facts are opposed to it; but for these also we must refer to our author.

---

* In Carinthia, which has no foundling hospital, 23·57 per cent. of children died under one year of age; in Krain, which has such an institution, only 20·36 perished.

The result of the careful inquiry into which Dr. Melzer has entered, seems to be unfavorable to the pretensions of what he designates as the Catholic system; and we are not the less inclined to accept the conclusion at which he arrives from its being couched in the language of the review to which we have referred. Foundling hospitals are mischievous, "because they have no influence on those evils which they were instituted to prevent; because the relief they proffer can be obtained only by a deliberate sacrifice of the best feelings of our nature; because they are liable to abuses, which it is almost impossible to prevent; and because, while they entail a great expense upon the country, they preserve the lives of but a very small proportion of their inmates."*

We know not whether the writer of these judicious conclusions would be disposed to go to the length to which we have indicated our disposition to advance in the matter of public charities; but we are so far disposed to generalize that we would extend our condemnation to all public establishments, which are not in the nature of *prevention*. If any species of public charity ought to be supported by the government, assuredly hospitals and dispensaries are such charities, and yet these have been largely provided by the benevolence of individuals; and though, not by any means free from some of the abuses of foundling hospitals, there can be little doubt that they are much more free from objection, than if they were public institutions supported by the public purse. The real position in life and wants of their inmates is much better known to the subscribers, and those who have the privilege of recommendation, than it could be to government officers; and thus a part, at least, of the abuses to which all charities are liable is guarded against.

---

* No. XXVI, April, 1842, p. 293.

We cannot conclude this notice of Dr. Melzer's admirable work, without again reverting to his censure of our countryman Malthus; and we cannot help regarding the conclusion at which he has arrived, respecting the evils of foundling hospitals, as applicable *mutatis mutandis* to all systems of poor-laws, against which our distinguished countryman so earnestly protested.

A short and simple proverb, PREVENTION BETTER THAN CURE, is at the root of all these questions. "Donnez plus de moralité aux classes ouvrières et il y aura beaucoup moins d'enfants trouves," (the concluding quotation of Dr. Melzer's work,) admits of extension in many directions. For instance, we would say to ENGLAND:—Give more intellectual and moral training, and you will have fewer criminals; commit less waste, and you will have fewer paupers; prevent disease and preserve health, and you will need fewer hospitals and workhouses; make the employers of labour responsible for life and limb, and you will have fewer accidents; look to the construction of ships and the education of masters, and you will have fewer shipwrecks; and so on through the length and breadth of your legislation. If this simple proverb will not suffice for your guidance, add this other homely saying, WASTE NOT, WANT NOT. If you will not take measures for preventing the refuse of our towns from flowing into the sea, then you must import manure, or corn and meat, or both. There is no other alternative. If you will not consent to that free competition of nation with nation, which, by stimulating all to exertion, makes all men prosperous, then must you take the consequences of your so-called protection,—sloth, ignorance, poverty, revenue laws, preventive service, law-made crimes, and all the waste of time and money which these entail. By your discountenance of foundling hospitals, you show that you are not ignorant of the true principles of political and Christian economy. We earnestly commend the poor laws to your best attention. You are the richest nation in Europe—you have the largest proportion of paupers. Can this be explained on any other suppposition than that they are *law-made* paupers? Try the system of *prevention*: it cannot fail more egregiously than the system of *cure* has done. The present system has the condemnation of the past: its rival has all the promises of the future.

Art. VI.

Observations and Essays on the Statistics of Insanity; including an Inquiry into the Causes influencing the Results of Treatment in Establishments for the Insane: to which are added, the Statistics of the Retreat, near York. By John Thurnam, Licentiate of the Royal College of Physicians of London, Resident Medical Superintendent of the Retreat, near York.—*London*, 1845. 8vo. pp. 308.

In the year 1841, a report, by Dr. Thurnam, including a series of statistical tables, exhibiting the practice of the Retreat, the asylum for the insane of the Society of Friends, for a period of 44 years, from its first establishment in 1796 to 1840, was printed for distribution by the directors of the institution. Of this valuable report a brief notice was inserted in our Journal at the period. The work now before us contains an enlarged and revised edition of the "Statistics of the Retreat," preceded by an elaborate investigation of the present state of the statistics of insanity, deduced from reports and other data furnished by hospitals for the insane in this country, on the Continent, and in America. This portion of the work, we are informed by the author, had its origin at the time he was engaged in drawing up the "Statistics of the Retreat," in the desire to compare the results afforded by the practice of that institution with those obtained in other asylums and hospitals for the insane.

The subjects treated of by Dr. Thurnam are arranged under three heads, the discussion of which is preceded by a concise exposition of the value of statistics as applied to the elucidation of insanity.

I. On the Statistics of Insanity in general. This may be regarded as constituting the body of the work, and is distributed in three chapters, which are devoted—1st, to the methods of deducing and exhibiting the results of treatment in institutions for the insane; 2d, to the various circumstances capable of influencing these results; and 3d, to the statistics of the principal asylums of this and other countries.

II. Essays on the liability to insanity in the two sexes, at different periods of life, and in the Society of Friends.

III. The Statistics of the Retreat, preceded by a sketch of its history and an exposition of its system of management, and of the methods of treatment pursued. To these are added, in an appendix, the history and statistics of the York Lunatic Asylum, and contributions to the statistics of the Society of Friends.

I. ON THE STATISTICS OF INSANITY IN GENERAL.

1. In order accurately to exhibit the results of the practice of different establishments for the treatment of insanity, it is necessary that the terms employed should be clearly defined; that the registers should be correctly kept; that the mode of calculating the proportion of recoveries and deaths should be uniform; and that the data for calculation should be deduced from observations extending over a sufficient period of time. To the first requisite it is not necessary further to allude: in reference to the calculation of the proportion of recoveries and deaths, various methods have,

however, been adopted in different institutions, and the periods of observation have often been too limited to allow of any precise comparison.

Dr. Thurnam, following the plan adopted by Mr. Farr, in his pamphlet on the "Statistics of English Lunatic Asylums," published about ten years ago, has calculated the proportion of recoveries on the number of cases admitted. To this plan it has, however, been objected, that it exhibits results less favorable than the true proportion, as of the whole of the cases, some still remaining under treatment may admit of cure, and yet have no account taken of them. Mr. Farr, accordingly, in his paper on the "Mortality of Lunatics," in the Statistical Journal for 1841, adopted the method of calculating the proportion of recoveries on the numbers discharged, and in the Reports of the Glasgow Asylum and those of the State Asylum at Worcester, Massachusetts, U. S., the same plan has been followed. This mode, we think with Dr. Thurnam, is still less accurate than the former, the results being much too favourable; as, of the cases received into any institution, a considerable number remain in a chronic state, to swell, at a later period, its mortality. Mr. Farr has, indeed, met this objection by stating that, "if the mortality remained the same, the probability is that the patients to be discharged would, *cœteris paribus*, be discharged cured, relieved, and died, in the same proportion as those already discharged." Our author, however, shows that the results of observations do not confirm Mr. Farr's theoretic statement—of the patients remaining in an institution at any period, a much less number being in a curable state than its average proportion of cures. Thus, at the Retreat, at Midsummer 1840, the proportion of cases regarded as curable was only 15 per cent., while the average proportion of recoveries during the 44 years that it had been in operation, amounted to nearly 47 per cent.

Under either of these methods the results are not strictly accurate, the true proportions being intermediate between those thus obtained. The former plan, however, probably possesses the greatest exactitude, and this view is confirmed by the fact, that while, according to the latter method, the proportion of recoveries at any institution is in its early periods high, and undergoes a rapid diminution, that furnished by the former gradually increases at each quinquennial period for a long series of years.

The most accurate method of estimating the average mortality of hospitals for the insane is, doubtless, as shown by Mr. Farr, to calculate the proportion of deaths on the average annual population or mean number of patients resident in the institution. In the reports of some asylums, however, the mortality has been estimated by the proportion of deaths to the patients admitted, and in others, to those discharged, while in some, amongst the most recent of which may be mentioned that of M. Parchappe, on the asylum at Rouen, the error has been committed of calculating the mortality on the total number resident during each year. Our author shows that our judgment of the character of these establishments, as founded on the rate of their mortality, will be entirely reversed according as the proportion of deaths is calculated on one or other of these plans, and, we need scarcely observe, that, without the employment of uniform methods of estimating the mortality, no satisfactory comparisons can be instituted.

In estimating the relative mortality of acute diseases, which have generally a definite course and duration, the period of residence is of little importance, but in an affection so chronic as insanity, and of which the duration is so varied, no comparison can be made between the average mortality of different institutions, if time be not taken into the account. Dr. Thurnam, therefore, justly expresses regret that in so few of the reports of asylums has the average population, or mean number of patients resident in the institution been hitherto given. We would farther observe, that the same omission exists in the reports of many of our general hospitals, in which, though of little importance as applied to estimates of the relative mortality of similar acute diseases, the average population is a necessary item in all comparisons of general results, and calculations of an economical nature.

To insure accuracy in the comparison of the mortality of lunatic asylums, it is also necessary that the estimate should be founded, not merely on the total number of patients resident, but on the numbers at the several periods of life respectively:—since it is evident that the varying mortality at different ages may materially affect the general results in institutions receiving different proportions of patients at the several periods of life.

All who are familiar with statistical inquiries are aware of the fallacious results of calculations deduced from limited series of facts and short periods of observation; the period, however, over which it is necessary that the observations should extend, in order to insure accuracy in stating numerically the results of the practice of lunatic asylums, appears, from our author's researches, to be greater than would *à priori* have been anticipated. At the Retreat, he informs us, that the period of twenty years elapsed from the establishment of the institution, before the proportion of cures arrived at the point which it has since maintained during the entire period of its operation. The rate of mortality during the first five years amounted to 5·71 per cent., and gradually declined from that period, till, at the end of 25 years, it averaged only 3·71 per cent.;—it then again underwent an increase; and, after the lapse of 35 years, attained the rate of 4·71 per cent., which it has since preserved. From an investigation of the statistics of other institutions, Dr. Thurnam has also found that the proportion of recoveries has, in nearly every instance, gone on increasing for a period often of 30 to 40 years from their commencement, owing probably, to the large number of chronic cases admitted at the first opening of any asylum, and to the long period occasionally required for the cure of the disease. On the other hand, it appears that the mortality is generally more favorable in the early periods of the operation of an asylum, and gradually increases for 20 or 30 years, when, as more recent cases are admitted, and the older ones die off, it often presents a rate of 50 or 100 per cent. higher than that of the first few years. Dr. Thurnam, therefore, suggests that, in the published reports of asylums, a summary should be appended of the general results for a series of years, dating from the establishment of the institution, or, if any great change have taken place in its mode of management, from the period of such change.

2. In treating of the circumstances in the character of the cases admitted and the method of treatment pursued, as influencing the results

obtained in hospitals for the insane, Dr. Thurnam adopts with some modification the arrangement of Gavarret, into—1st, individual conditions; 2d, hygienic conditions anterior to the invasion of the disease; 3d, the form of disease, its duration, &c.; 4th, the hygienic condition during treatment; and 5th, the treatment itself.

Of the individual conditions and the hygienic conditions anterior to the invasion of the disease, the chief circumstances which modify the results of treatment are the sex and age, rank and previous habits of the patients, and the predisposing and exciting causes of the disease.

Sex. Writers have differed as to the relative frequency of recoveries from insanity in the two sexes. The generally received opinion has however been, that in females the probability of recovery is not only greater, but that the tendency to fatality is less. With these views Dr. Thurnam's conclusions coincide. He states that, with two exceptions, in all the institutions the statistics of which he has examined, and of which the periods of operation have been sufficiently extended, the proportion of recoveries in females has exceeded, often to a considerable extent, that in men. The excess in favour of females he finds to vary in different institutions, from 4, 5, and 7 per cent. in the asylums of Glasgow, Belfast, and Lancaster; to 20, 22, 23, and 28 per cent. at the Retreat, the asylum of Schleswig in Holstein, Charenton, and the York Lunatic Asylum. At the Senavra hospital, in Milan, the proportion of recoveries in females has only exceeded that in males by 2 per cent., and at Hanwell and the Bloomingdale Asylum, in the State of New York, U. S., the excess of recoveries is on the side of males, and amounts to 5 and 28 per cent. respectively. The results afforded by the experience of the Senavra may probably, it is suggested, be referrible to the cases treated in that institution being most generally complicated with the peculiar endemic of Lombardy—the pellagra. The great excess of recoveries in males at the Bloomingdale asylum is ascribed to persons labouring under delirium tremens being treated in that institution.

The difference between the mortality of insanity in the two sexes is greater than in the relative probability of recovery. Instead of the excess of 5 or 6 per cent., which obtains in the general population, our author's investigations show that, with the solitary exception of that of Belfast, the average mortality among males in the British asylums is uniformly very much greater than among females. At the York Lunatic Asylum and St. Luke's there has been an excess on the side of the men of 93 and 98 per cent. respectively; and, in the institutions where the difference is least, as in the asylums of Schleswig and Worcester, U. S., the mortality in males is 9 and 13 per cent. greater than in females. This difference applies to institutions admitting all ranks of patients in a nearly equal degree; being in the metropolitan licensed asylums for paupers 63 per cent.; and in those for private patients 57 per cent. At the Senavra, the mortality in females, during a period of 25 years, exceeded that in males by 13 per cent. This statement is sufficient to show the necessity for giving the average mortality of any institution in each sex separately, as it is evident that, by the respective proportion of males and females, the general results will be materially modified.

As regards the liability to relapse and recurrences of the disorder, the influence of sex would appear to be partially reversed, females being somewhat more frequently the subjects of second attacks than men. The results obtained by our author on this point, as also on the frequency of second attacks of insanity, being highly interesting, and opposed to the views generally entertained, we shall lay before our readers. The method of ascertaining the frequency of the recurrence of insanity which has ordinarily been adopted, has been to calculate the proportion which the cases readmitted into any institution bear to the whole of those treated or discharged cured. Esquirol was thus led to estimate the relapses at 10, Pinel and Despertes at 17, and Farr at 30 per cent. of the recoveries. It is, however, obvious that a calculation of this kind is open to the fallacies that cases may be readmitted which were not recovered at the time of discharge, and that cases discharged cured may relapse, but not be again sent to the institution. At the Retreat the readmission of cases discharged recovered have amounted to 31·5 per cent. This must, however, our author remarks, be regarded as less than the true proportion of relapses; as, of those discharged, some may yet, after several years have elapsed, experience second attacks. In support of which, he states that five men, labouring under second attacks, had been admitted since the date of the report, and after 4, 5, 8, 14, and 19 years had respectively intervened since the period of their recovery from the first attack. A further correction must be made for those who, having been discharged from the institution cured, were known to have experienced subsequent attacks, but had not been readmitted. Dr. Thurnam, therefore, infers that, by this method of investigation, "the liability to relapse or recurrence of insanity, after recovery from a first attack, all things considered, can scarcely be estimated as at all less than 50 per cent., or as 1 in every two cases discharged recovered."

As stated by Mr. Farr, it is, however, only by following through life a large series of cases where recovery has occurred, that the frequency of second attacks of insanity can be satisfactorily ascertained. The Retreat affords peculiar facilities for such an investigation; as "not only is almost every case of second attack readmitted, but an opportunity may generally be found for ascertaining the subsequent history of all cases that have at any time been under care." Dr. Thurnam has thus been enabled to obtain full information as to the whole of the 244 persons who have at any time been patients in the institution, and who have subsequently died. Of these it appears that only 131, or 53·6 per cent. recovered from the first attack; and of these 45, or nearly one third (18·4 per cent. of the whole) continued permanently free from mental disorder, while 86 had one or more subsequent attacks, and only 20 were sane at the time of death. Of the 244 cases, therefore, only 65 (45+20), or 26·6 per cent. died sane.

> "In round numbers, then, of 10 persons attacked by insanity, 5 recover and 5 die, sooner or later, during the attack. Of the 5 who recover, not more than 2 remain well during the rest of their lives; the other 3 sustain subsequent attacks, during which at least 2 of them die. But although the picture is thus an unfavorable one, it is very far from justifying the popular prejudice, that insanity is virtually an incurable disease; and the view which it presents is much modified by the long periods which often occur between the attacks, during which intervals of mental health (in many cases of from 10 to 20 years' duration) the individual has lived in all the enjoyments of social life."

Our author's observations show that the proportion of recurrences in women is somewhat greater than in men; being, in the former, 67·1, in the latter, 65·7 per cent.

Age. The greater curability of insanity in early life has been long established. Esquirol regarded the disease as most amenable to treatment in persons between 20 and 30 years of age. This result differs somewhat from the conclusions of our author; who, from the tables of the Retreat, York Asylum, and Bethlem Hospital, infers that the recoveries are most numerous at the earliest ages at which the disease prevails, and gradually decrease in number with the progress of life. The proportion of the recoveries under 20 years of age being 55·55 per cent. of the whole of those admitted between these ages; 51·77 per cent. between 20 and thirty years; 46·16 between 40 and 60; and 27·8 between 60 and 80. The decrease in the proportion of recoveries from 40 to 60 and 80 years of age, follows therefore, a rapidly increasing ratio, being 7, 11, and 40 per cent. respectively. The only exception to the increasing proportion of recoveries with the progress of age, is stated to exist in females; in whom the proportion of recoveries at 10 and 20, and 40 and 50, is less than in the subsequent periods of 20 and 30, and 50 and 60 years of age; exceptions of which the peculiarities of the sex afford a probable explanation. The mortality of the insane at different periods of life has been made the subject of an elaborate investigation by our author; and the table which he has compiled from the experience of the Retreat and York Lunatic Asylum, constitutes, we believe, the only approach to a life-table for the insane which has yet been published. The rate of mortality, as observed at the Retreat, has been adopted by the Medical Invalid Life Assurance Society, for the calculation of the rates of insurance on the lives of insane persons under favorable circumstances. From the table showing the estimated mean annual mortality at these institutions, as compared with that in the population at large, and in the Society of Friends, it appears that, supposing 100 persons were living at each decennial period of life, from 20 to 90 years of age, instead of the mean mortality at all ages being, as estimated by Edmonds, for the country at large, 6·03 per cent., or as deduced from the data given in the Appendix for the Society of Friends alone, 4·8 per cent.—the mortality of the York Lunatic Asylum is found to be 10·88, and that of the Retreat 7·32 per cent. While, however, there thus exists a great excess in the mortality of insane persons at all ages over that of the population at large, this excess is by no means equal at each period of life, being much greater at the early ages than in more advanced life. Dr. Thurnam remarks that—

> "The mortality at the Retreat, especially among males, from 20 to 50 years of age, was double, treble, and even quadruple, what it is among the sane at the same ages; whilst at 70 years of age and upwards, it is only slightly, if at all, higher than in the population at large. And thus it appears not improbable that if the insane survive the 60th year, they are nearly as likely to attain advanced age as the sane. At this period of life, the disorder has in general passed into a fatuity, or, at least, into a chronic and often lively form of insanity; and the bodily functions are often remarkably active, if not also vigorous. The prevalent error of the insane being long-lived is, perhaps, to a certain extent, to be explained by this interesting and unexpected result."

The influence of insanity on the duration of life is further illustrated by tables giving the age of the patients, the duration of the disorder, and the period of death, in the whole of the cases which have proved fatal at the Retreat since its establishment. These cases are 139 in number, and the average age at which such of the patients as were members of the Society of Friends were attacked with the disorder was 39·19 years; of the others, 38·6 years; and the mean age at the period of death was 56·5 and 47·7 respectively. In reference to these results, our author remarks that, as at 38 and 39 years of age, the expectation of life is regarded as not less than 28 years, the mean ages attained should have been 67 and 66, instead of 56 and 47 respectively. "In those connected with the Society, therefore, less than two thirds, and in others not more than a third, the expectation of life at the time of attack was, on an average, realized." Notwithstanding the occasional occurrence of instances of longevity amongst insane persons labouring under the chronic form of the disease, the tendency of insanity to shorten life is thus shown to be very decided, owing to the high rate of mortality in the earlier stages and more acute forms of the disease. It is further stated, that in those who recovered from first attacks, and died subsequent to discharge, the ages at the time of death and at the period of attack corresponded with those observed in the institution; evincing that after recovery from insanity, as after attacks of most important diseases, the average duration of life is materially shortened.

3. Few subjects have recently attracted more deserved notice than the influence of distress and destitution, and their attendant evils, in increasing the prevalence of disease, and lessening the average duration of life; and it was to be anticipated that the experience of asylums for the pauper population, more especially of the large commercial and manufacturing towns, would present results less favorable, both as regards the recoveries and mortality, than institutions wholly or in part devoted to the reception of persons of the more opulent classes. This inference is confirmed by our author's researches; and he further thinks it probable "that the proportional difference in the rate of mortality in different classes of society is much increased under the operation of insanity, and that it is greater than under that of disease in general." In support of this opinion he contrasts the proportion of recoveries and mortality at the Retreat with their rate at the asylums of Wakefield and Lancaster, for the paupers of the West Riding of Yorkshire and Lancashire. In the former institution the proportion of recoveries, in persons connected with the Society of Friends, has been about 50 per cent., and the mean mortality 4·7 per cent.; whilst at Wakefield and Lancaster the recoveries have only been 43·6 and 41·4 per cent., and the mean mortality has attained the high averages of 15·7 and 16·5 per cent. respectively. A similar, though less marked, difference is observed in the metropolitan licensed private asylums; the mortality having been 20·68 per cent. in such of the houses as receive paupers, whilst in those for private patients it was only 10·94 per cent. In reference to the remarkable contrast thus exhibited, Dr. Thurnam remarks that it can scarcely be entirely attributed to the previous circumstances of the patients, but must, in part at least, be referred to the pauper patients not enjoying an equal degree of domestic comfort and as nutritious a diet as the rich.

Passing over the fourth and fifth classes of circumstances modifying the results of treatment in asylums for the insane, we shall give our author's conclusions as to the influence exerted on the statistics of such institutions, by the duration of the disorder at the period of admission, and by the duration of residence or treatment.

The duration of the attack at the time of admission into hospitals for the insane exerts a double influence on the statistical results;—the probability of recovery being much greater in such cases as are brought under care at an early period, while the average mortality is less in the chronic stages of the disease.

> "At the Retreat, the probability of recovery in cases brought under care within three months of the first attack, has been found to be as 4 to 1, and excluding cases complicated with serious bodily disorders, as 9 to 1; whilst in cases not admitted till more than twelve months after the attack, the probability of recovery is less than as 1 to 4. The proportion of recoveries now mentioned as having occurred at the Retreat, in uncomplicated cases of less than three months' duration, or that of 9 out of every 10 cases brought under care, is the same as that which, in cases equally recent, the late Dr. Willis and Dr. Burrows both stated as having occurred in their private practice. Though, both when first made, as well as since, Dr. Willis's statement has often been called in question, it may still be regarded as not in any degree improbable, and may, indeed, often apply to a small number of cases. But unless a selection be made of uncomplicated cases, it may be questioned whether so large a proportion will be found to recover when the experience is not a limited one. Much reliance, indeed, should never be placed upon the results derived from a limited sphere of observation, whether in public or private practice, as proof of which it may be here stated, that out of twenty cases admitted at the Retreat within three months of the first attack, during 10 years (1798-1808), 19 were discharged recovered."

As before stated, the short duration of the disorder on admission exerts an unfavorable influence on the rate of mortality. During 48 years, the annual mortality at the Retreat averaged 7·3 per cent. in cases of first attack admitted within three months of the commencement of the disorder, and only 4·7 per cent. in cases whether of first attack or otherwise, and of more than twelve months' duration. These differences in the results of treatment in cases of insanity, according to the period at which they are admitted into hospital, show that to enable any satisfactory comparisons to be made between the results of different institutions, it is necessary that similar modes of classification of the cases as to duration at the time of admission should be adopted. The cases at the Retreat are arranged into four classes—1st, cases of the first attack, if not more than three months' duration at the time of admission; 2dly, cases of the first attack of more than three, but not more than twelve months' duration; 3dly, cases not of the first attack, and of not more than twelve months' duration; and, 4thly, cases whether of the first attack or not, and of more than twelve months' duration. This plan, or one almost identical with it, our author informs us, has already been introduced into several of the county asylums, and he suggests that a similar classification should be generally followed.

The duration of the treatment, or residence, may be modified by a variety of causes; as by the results of treatment being more or less favorable, and so having a smaller, or larger, proportion of cases in an incurable condition; by economical motives occasioning an early removal of the patients from the institution; or by special rules limiting the duration of their residence. At the York Lunatic Asylum, where the second cause operates, the patients, on an average, only remain 2½ years (2·52); at Bethlem and St. Luke's, where they are only permitted to remain twelve months, the duration of residence of curable cases averages only half a year (·57), and two-thirds (·68) of a year respectively. While at the Retreat, where, from the peculiar constitution of the Society of Friends, neither economical nor other motives operate to hasten removal, the mean period of residence has been nearly five years (4·8). In the latter institution one-third (34 per cent.) of the entire recoveries have occurred after the first year of residence; and nearly one-sixth (or 15·3 per cent.) after the second year. Though, therefore, at the Retreat, in some of the cases the period of probation may have been unnecessarily prolonged, it is evident that the short duration of residence at the former institutions must operate unfavourably on their proportion of recoveries, and, by occasioning the premature discharge of cases, must lessen the probability of ultimate recovery. The long duration of residence at the Retreat not only proves favorable to its proportion of cures, but lessens the average mortality.

The influence exerted on the statistics of hospitals for the insane by the several particulars of treatment, is treated of under the two heads of the general hygienic conditions during treatment, and the treatment itself. Of the former class of modifying causes are—1st, the healthiness of the locality in which the asylum is situated, as influenced by climate, elevation, soil, drainage; 2d, general adaptation and appropriate construction of the building; 3d, means of exercise, occupation, and amusement; 4th, internal economy and government, number of attendants; 5th, ventilation, lighting, warmth, and cleanliness of the apartments, as modified by the numbers treated in the institution; 6th, clothing and personal cleanliness, baths, &c.; 7th, diet. Of these various circumstances our author takes a rapid survey, and weighs the probable effect of their good or bad arrangement on the proportion of recoveries and mean annual mortality of institutions for the insane. In reference to the internal economy and government of asylums, he agrees in the opinion, which we believe to be general at the present day, that the most efficient system of management is that in which the entire direction is vested in a resident medical officer, assisted by the advice of visiting physicians. Such a system, to be thoroughly carried out, presupposes the limitation of asylums to the accommodation of a moderate number of patients. Dr. Thurnam quotes on this subject the opinion of Jacobi, that the maximum number of patients to be admitted into one asylum ought never to exceed 200; probably, however, in pauper asylums, accommodation for 300 or 400 patients is not too great. Our author also expresses his approbation of the suggestion to erect *asylums* for the detention and care of the incurable and harmless patients, apart from the *hospitals* for the treatment of the more acute and severe cases of insanity; and suggests that such institutions should be in the immediate neighbourhood of the latter, and under the charge of their medical officers. We believe that this change, more perhaps than any other, would conduce to further the efficiency of our larger public asylums; many of which are at present so crowded with patients in a state of fatuity and in the chronic forms of the disease, as to limit the accommodation for the more acute and urgent cases, and interfere materially with their successful treatment.

The number of attendants on the patients in institutions for the insane is of great importance, as affecting not only the comfort of those under treatment, but influencing a variety of circumstances by which the general efficiency of an establishment, and its consequent proportion of recoveries, and amount of bodily disease and mortality, are materially modified. The proportion which the attendants bear to the patients in different asylums seems to vary considerably. In 13 county asylums, in the year 1835, the average proportion was 1 attendant to 17 patients; and, in some of these establishments the number was much less. In the Wakefield Asylum there was only 1 attendant to 20 patients, and at Hanwell 1 to 26; but the proportion has since been increased in the latter institution to 1 to 18. In 8 of the 10 Irish district asylums there is 1 attendant and assistant to 9 patients; or, taking responsible attendants only, 1 to 13 patients. At the York Lunatic Asylum and the Lincoln Asylum, which receive both pauper patients and those of the upper ranks of society, the proportion of attendants is 1 to 11 or 12, and 1 to 9 patients. In the asylums of Montrose, Aberdeen, Glasgow, Dundee, and Perth, the proportion appears to average 1 to 10. At Siegberg, near Bonn, it is 1 to 7 or 8; and at the Retreat, 1 to 6 or 7 patients. Our author concludes the enumeration of the proportion which the attendants bear to the number of patients under treatment at different institutions by remarking that—

> "In pauper *hospitals* for the insane, in which the patients are frequently able to render considerable assistance to the attendants, the proportion of the latter will not, perhaps, on the average, require to be more than 1 to 12 or 15; and in *asylums* for this class the proportion need not be half even this. In other institutions, the highest class of patients, on account of their previous habits, will often require a separate and distinct attendant to each case. This should, perhaps, always be the case in the early stages and more severe forms of the disorder; but in the more advanced stages, and more chronic and harmless forms of insanity, a limited number of this class, not exceeding five or six, may be properly enough associated together under the care of two or three attendants."

The diet of the patients in the various county asylums for paupers differs considerably, both in the quality and quantity of the food; and, in some, the unfavorable results of treatment, as evinced by the low proportion of recoveries and high rate of mortality, is probably in part ascribable to a deficiency in the amount of nutritious food allowed. With a view to illustrate this subject, our author has given a table of the diet in seven of the principal county asylums, as it existed in 1835. These institutions, he shows, may be divided into two groups; in one of which, including the asylums of Nottingham, Stafford, and Gloucester, the diet was considerably above the average of similar institutions; while, in the other group, embracing the four asylums of Wakefield, Lancaster, Suffolk, and Middlesex, the allowance of food was much below the average. In the first group, the solid food, consisting of meat, cheese, puddings, bread, and other articles, exclusive of vegetables, amounted to 225 oz. per week; while in the second it was only 150½ oz. The amount of animal food alone was, in the first group, 46 oz., and in the second only 19½ oz. per week; and, though the allowance of soup, porridge, milk, &c., was in the latter 15½ pints per week, while in the former it was only 10 pints, this excess could by no means compensate for the deficiency of more solid nutriment. In the first group, also, the quantity of beer allowed amounted to 2 pints daily; in the latter to not more than half a pint. The difference in the results as to recoveries and deaths between these two groups of institutions is very striking; in the three asylums with the more liberal diet, the recoveries having been 43·7 per cent., and the mean mortality 9·35 per cent.; while in the second group, with the more scanty allowance of nutritious food, the recoveries have averaged only 36·75 per cent., and the mortality has been as high as 14·54 per cent. It would be premature, on the limited information before us of the circumstances of these several institutions, to draw any positive inference as to the dependence of their comparatively successful treatment on the respective diets allowed; it seems, however, not improbable that a portion, at least, of the discrepancy may be referred to this cause. The observations call for a fuller investigation into the effect of diet on the treatment in pauper asylums; and it would be well that in these institutions, as in workhouses and prisons, fixed dietaries, apportioned on a liberal scale to the several classes of patients under treatment, and convertible so as to present varieties in the kind of food allowed, should be arranged by the commissioners, and required to be generally observed.

The remaining portion of this section of Dr. Thurnam's work is devoted to the consideration of the several objects to be held in view in the treatment of insanity, and the influence of the treatment pursued on the statistical results of hospitals for the insane. The section concludes with an illustration drawn from the history of the York Lunatic Asylum, prior and subsequent to its reformation in 1814. It is impossible to read this and other accounts of the state of our asylums a few years ago, without feelings of great gratification with the changes which have taken place. In reference to this institution, it is satisfactory to find that the abandonment of a system of management fraught with cruelty and abuse, and the adoption of plans of treatment more accordant with humane feelings, have been attended with a very decided improvement in the health of the inmates, as evinced by the decrease in the average mortality. Thus our author shows that, during the 37 years from the establishment of the institution to the period at which the change in the management took place, the average mortality was 11 per cent.; and during the six years which immediately preceded that change, and when the abuses in the asylum had attained their greatest height, not less than 14·8 per cent. Since this period, on the contrary, in the 26 years which elapsed to 1844, the mortality has been only 7·24 per cent., or 40 per cent. less than its rate during the entire former period, and fully 100 per cent. less than during the six years preceding the change. The mode in which the registers of the patients were kept before the reform precludes the possibility of any investigation of the relative proportion of recoveries; but there can be no doubt that the improvement in this respect has not been less satisfactory.

The third and concluding chapter of this portion of Dr. Thurnam's work contains the statistics of the principal hospitals for the insane in this country, on the Continent, and in America. An elaborate table presents us with the data furnished by the entire experience of 59 public asylums, including the observation of more than 125,700 cases of insanity; together with calculations of the proportion of recoveries in the numbers admitted, and the mean annual mortality estimated on the average population. We quote the following tabular statement of the results deduced from this laborious investigation:

	Proportion of recoveries.	Mean mortality.
9 English county asylums, receiving paupers	36·95	13 88
6 English county asylums, receiving paupers and private patients	46·87	10·46
8 English asylums, supported by charitable contributions, and receiving pauper and private patients	40·94	8·93
7 Scotch asylums, receiving pauper and private patients	42 37	7·52
10 Irish district asylums for paupers	48·33	8·7
5 asylums in the United States for paupers and others	46·82	9·56

From this table it is inferred that, in asylums which have been established for a considerable length of time, the proportion of the recoveries to the whole of the cases admitted should be considered low, if amounting to much less than 40 per cent., and high if exceeding 45 per cent. On the other hand, the mortality in an asylum for the more opulent classes of society, as well as for paupers, calculated on the average numbers resident, may be considered highly favorable if under 7 per cent., and decidedly unfavorable if exceeding 9 or 10 per cent. In asylums wholly appropriated to paupers, a mortality of much less than 10 per cent. must be regarded as very low, and one exceeding 12 or 13 per cent. as decidedly high. While, however, the sources of fallacy in estimating the relative efficiency of different institutions by the average mortality in the resident population are fewer than those which attend conclusions drawn from the proportion of recoveries, great caution must be exercised in adopting either of these standards of comparison.

II. ESSAYS ON THE LIABILITY TO INSANITY.

1. *Relative liability to insanity in the two sexes.* It has been the generally received opinion since the time of Aretæus and Cælius Aurelianus, that females are more subject to insanity than males, and this Esquirol, from an extensive statistical investigation, believed to be correct, inferring that 38 females became insane to 37 males; a view which has since been maintained by most writers on insanity. In estimating, however, the relative liability of the two sexes to this disease, Esquirol took no account of the relative proportion of the sexes living at the periods of life when insanity is most frequent; an omission which, with the known preponderance of females in the general population above 15 and 20 years of age, goes far to explain the results he obtained.

In this country Dr. Thurnam states that the only institutions which maintain a much larger proportion of females than males are those in the neighbourhood of the metropolis, Bethlem, St. Luke's, and Hanwell; and in the latter there has been of late years a gradual diminution of the excess in females. Their greater proportion in these asylums may perhaps be explained by the preponderance of females being still greater in the population of London than in that of the country at large, amounting in the former to 118 females to 100 males from 20 to 50 years of age, while in the latter there are only 108 females to 100 males. The greater proportion of insane females in these institutions may also be in part referrible to peculiar circumstances connected with the metropolitan population. The excess seems only to exist in the inferior ranks of society, as in the licensed metropolitan asylums for private patients, males are in the largest proportion.

Esquirol also, in his statistical investigation on this subject, estimated the relative proportion of the two sexes in the *existing*, not in the *occurring*, cases of insanity; a method of inquiry which, when the relative mortality of the disease in the two sexes is considered, is obviously fallacious. Dr. Thurnam has shown that, while in the general population of this country the mortality of adult males exceeds that of females only by 7 or 8 per cent., the excess of mortality in insane persons of the male sex is not less than 50 per cent., and consequently that the gradual accumulation of incurable females in our asylums is, in this proportion, greater than that of males. It is obvious, therefore, that the question of the relative liability of the two sexes to mental disease must be decided by the relative proportion of occurring cases in each; or, as the only available means of ascertaining this, by the relative numbers admitted during a term of years into our hospitals and asylums for the insane. With this view, Dr. Thurnam has compiled a table of 48,103 cases admitted into 32 different asylums in this country, America, and on the Continent, from which it appears that in 24 of these institutions the admissions of males were the most numerous, the excess in the whole number amounting to 13·7 per cent., and in some of the English institutions being not less than 25, 30, and 40 per cent. The only English county asylums where this excess does not exist are those of Dorset and Norfolk. From a still larger number of cases, including persons of all ranks of society, he elicits similar results; and it would appear from the greater excess of males admitted into the asylums of the metropolitan district for private patients, that in the upper ranks of society men are more liable to insanity than women, in a still higher proportion than obtains in the pauper population. It must, however, be borne in mind, that among the wealthier classes a larger portion of insane females may possibly be detained under the charge of their friends.

The experience of the Society of Friends, as ascertained by the admissions into the Retreat, would appear at first sight opposed to these conclusions; the excess of females admitted since its commencement amounting to 18 per cent. It is, however, probable, that amongst adults the excess of females in this community exceeds that of males in a larger proportion than in the general population, being, our author infers from data with which he presents us in an appendix, not less than 30 to 35 per cent.

In most respects, therefore, as regards insanity, females are more favorably circumstanced than males. They are less liable to the disease; and when seized, the prospect of their recovery is greater, and the tendency to death less; they are, however, apparently somewhat more exposed to relapses and recurrences of the disorder.

2. *On the relative liability to insanity at different ages.* Esquirol, proceeding on the same erroneous method of statistical analysis, as in estimating the comparative frequency of insanity in the sexes, inferred that

the tendency to the disease underwent a constant increase with the progress of life. Quetelet, on the contrary, concludes that insanity is most frequent between 40 and 50 years of age, and Burrows between 30 and 40.

It is evident that to estimate correctly the relative liability to insanity at different ages, a comparison must be instituted as to the number of *occurring* cases at the different periods of life, and the relations which these bear to the numbers living at the same ages in the general population. The materials which at present exist for instituting a comparison of this kind appear, from our author, to be scanty. The only published data on the subject, extending over a considerable period of time, being prior to those given in the 'Statistics of the Retreat,' a list of 566 cases, by Dr. Jessen, of Schleswig, and the report of Dr. Woodward, of Massachusetts, U. S. Dr. Thurnam, from these and other data, has compiled a table, showing the age at attack in 5122 cases; and a second, from the reports of the Hanwell Asylum for 5 years, 1840-44, from which he infers that the greatest liability to insanity is between the 20th, and 50th and 60th years. The attacks being most common between the 30th and 40th years, and undergoing after this period of life a gradual decrease; a conclusion opposed to that of Esquirol, but harmonizing with the *à priori* inference, that persons at the period of life when the intellectual and physical energies are at their height, and exposure to the influence of disturbing causes the greatest, would be most subject to the disease.

In some of the American asylums, statistical observations show that there is a greater prevalence of the disease between 20 and 30 years of age than at any other period; a fact referrible probably to the peculiar circumstances of the population of that country. The same, from the observations made at the Retreat, would appear to be the case in the Society of Friends. The proportion of cases admitted between 20 and 30 years of age being nearly one-third of the whole, and the admissions after this period of life declining.

3. *Liability to insanity in the Society of Friends.* Our author in entering upon the discussion of this subject, refers to the difficulty of estimating the liability to and prevalence of insanity, either in the general population, or in a particular community like the Society of Friends. In order to determine the liability to disorder in any community, he shows that as in the former instances the calculation must be founded on the number of occurring cases, as a variety of causes affecting the curability of the disease, the tendency to relapses, and the proportion of deaths, will materially affect the number of insane persons living in a given population. From the observations made at the Retreat, it appears that for the 20 years, 1820-40, the number of admissions averaged 12·35 annually; of which 5·45 were males and 6·9 females. Returns recently obtained show also that the mean population of the Society during this period was about 17,900, of whom 8137 were males and 9763 females. From these data it appears that 6·9 cases of insanity are admitted into the Retreat annually out of a population of 10,000 persons. It is further assumed that 2 other cases of insanity occur annually among members of the Society which are not sent to the Retreat, giving a total of 8·9 cases; or, making a deduction of cases readmitted into the Retreat which had not been discharged *cured*, of 8·1 cases out of 10,000 persons, or 1 in

1234. Of the 274 cases received into the Retreat during the twenty years, 163, or nearly two thirds of the whole, viz. 79 males and 84 females, were admitted with first attacks; deducting therefore the cases readmitted, Dr. Thurnam concludes that the actual cases which occur annually in the Society average 5·5 per 10,000 persons, or 1 in 1790.

The cases actually admitted into the Retreat and similarly corrected, amount to 4·55 per 10,000, or 1 in 2196. The males being 4·55, the females 4·3; showing an excess of males, as compared with the relative number of the two sexes in the Society at large, of 13 per cent.

At the asylum of the Society of Friends, at Bloomingdale, near Dublin, the cases admitted are in the proportion of 6·28 annually to a population of 10,000, or 1 in 1590 persons. At the Friends' Asylum, Frankford, U. S., they amount to 7·9 per 10,000, or 1 in 1265 of the population. In these instances it is not known what correction should be made for the cases not sent to the institutions; but the proportion admitted into the Frankford Asylum may perhaps justify the "surmise that mental disorders are somewhat more frequent in the Society of Friends in the United States than in the same community in Great Britain and Ireland."

For these statistics of the prevalence of insanity in the Society of Friends our author claims only an approximative value; they are, however, we may conclude, from the facilities for obtaining information on this subject afforded by the limited population of the Society, more trustworthy than any calculations we possess as to the frequency of the cases in the general population of this country. Till more accurate information in reference to the latter be obtained, the question whether, as supposed by Dr. Burrows, the members of this community be especially subject to attacks of insanity, or whether, as asserted by Dr. Haslam, they enjoy a peculiar immunity from mental diseases, must remain *sub judice.* The infrequency, however, of habits of intemperance among them, which in the population at large forms so fruitful a source of insanity, renders it probable that the prevalence of the disease in the Society of Friends will prove to be below the average in the general population.

The number of existing cases in the Society, as ascertained by the admissions into the Retreat, and corrected for cases not sent to that institution, appears to be in the proportion of 525 to 10,000 of the population, or 1 in 190 persons. This is undoubtedly a high proportion, but it is probably referrible to the favourable hygienic conditions in which the insane of the Society of Friends are placed; and, as suggested by Dr. Thurnam, to slighter cases being placed and continued under treatment than is generally the practice.

III. HISTORICAL SKETCH AND STATISTICS OF THE RETREAT.

Of the concluding division of Dr. Thurnam's work, space will only allow of our offering a very concise notice. The suggestion for the establishment of an institution by the Society of Friends, to be specially devoted to the reception and treatment of the insane members of that community, emanated from the late William Tuke, who, together with his son Henry Tuke and Lindley Murray, were the most active agents in its formation. To all who are familiar with the state of our asylums for the insane prior to the establishment of the Retreat, and with the influence which the mode of management adopted in this institution has had in the diffusion of more humane methods of treatment, the names of the Tukes, and especially of Samuel Tuke, the present treasurer and author of the 'Description of the Retreat,' published in 1813, will ever be held in esteem.

> "By a singular and interesting coincidence," remarks our author, "it was in the spring of 1792, the very year in which the celebrated Pinel commenced the amelioration of the treatment of the insane in France, by the truly courageous act of unchaining nearly fifty supposed incurable and dangerous lunatics at Bicêtre, that the establishment of the Retreat was proposed to the meeting referred to by the late William Tuke. The proceedings of Pinel, however, were not, until long after, known either to the directors or managers of the Retreat; and it thus appears that the reformation in the treatment of the insane had an independent origin in the two countries at the same period."

The institution was first opened for the reception of patients in 1796, and the committee appear to have been singularly fortunate in the choice of its first officers, Dr. Fowler, the visiting physician, and the late George Jepson, the resident superintendent. The testimony to the merits of the latter individual we regret our inability to transcribe in full. "Rarely, we believe, has it happened that a project so enlightened has met with an agent so efficient for the carrying of it out."

We shall conclude our notice by quoting the report of our author on the experience of the medical officers of the Retreat, on the *quæstio vexata* of this branch of practical medicine—the employment of means of personal restraint:

> "The subject of coercion during a refractory, violent, or otherwise dangerous state, or, in other words, the personal restraint of the insane by mechanical means, has of late attracted much attention. At the Retreat, from a very early period, it has been regarded more or less in the light of a necessary evil; and it has been one of the objects of the managers of the institution to resort to it as seldom as possible. The treasurer of the institution, who published his 'Description of the Retreat,' so early as 1813, when detailing the means of personal restraint then employed, observes, 'with regard to the necessity of coercion, I have no hesitation in saying, that it will diminish or increase as the moral treatment of the patient is more or less judicious.' But he immediately adds, 'we cannot, however, anticipate that the most enlightened and ingenious humanity will ever be able entirely to supersede the necessity for personal restraint.'
>
> "In 1841, when the first edition of the 'Statistics of the Retreat' was printed, I made the following observations on this subject:—'Within the last two years the officers of some institutions have attempted, and in some instances apparently with great success, to conduct the management of even large hospitals for insane paupers, without resorting to such means of restraint. The important experiment of this description, if experiment it can now be called, which Dr. Conolly is conducting at Hanwell, must on all hands be regarded with extreme interest, as even if it fail in establishing that personal restraint can in all cases be abolished, it has already fully shown that it may be much more frequently dispensed with, not only with safety, but with advantage to the patient, than has hitherto been generally, if at all suspected. The officers of the Retreat have not hitherto thought it right, in every case, to dispense with the use of all mild and protecting means of personal restraint; believing that, independently of consideration for the safety of the attendant, they may, in some instances, be regarded as the least irritating, and therefore the kindest method of control. But though this is the case, they readily admit that they have derived advantage from the full conside-

ration of the subject, which the attempts at Hanwell, Lincoln, and elsewhere have induced, and that they remain open to further evidence upon the subject.'

"It is now more than two years since the foregoing was written, and I have the satisfaction of adding that, in practice, personal restraint has by degrees been almost entirely abolished. Whilst we hold ourselves free to direct the use of any means which the necessities of the particular case appear to call for, I am fully convinced that, in a well arranged and properly governed public institution, the instances where personal restraint can at all be considered needful are in truth very few, and that they will be found, almost exclusively, to consist of old or mismanaged cases.

"I have indeed no hesitation in stating what can hardly I think be doubted, that restraint of every description, not absolutely called for, has a tendency to excite in the insane the angry and vindictive passions to which they are only too prone, and thus to prolong the continuance of the disorder in curable cases, and to aggravate its character in incurable ones. The instructions to the attendants on this subject have already been given; and during the current year there has only been a single example of the application of personal restraint, and that was of the mildest possible kind. There has been no instance of its application since the 25th of January, 1843."

In a note at p. 105 of the first part, we are further informed that in 4 cases slight personal restraint had been had recourse to between the 25th of January, 1843, and the 8th of February, 1845—and in a letter recently published in another periodical, it is stated that no instance of its employment in any form has since occurred; so that the use of coercion in the institution may be regarded as virtually abolished. Dr. Thurnam adds—

"Whilst on the one hand I cannot doubt that the course and duration of many cases have been mitigated and shortened, and the character of the disorder rendered less virulent, by the disuse of restraint, I must here state that occasional inconvenience in the shape of alarm, and of interruption to the quiet of other patients, and also as regards the destruction of clothing and the breakage of glass, have, on the other hand, been connected with it. On the whole, however, that greater vigilance and forbearance on the part of the attendants, which under competent superintendence the comparative disuse of personal restraint more or less necessarily implies, have, I feel no hesitation in saying, been attended with a decided increase of comfort and decrease of irritation in those divisions of the establishment in which instruments of restraint were formerly not unfrequently resorted to."

We must now take our leave of Dr. Thurnam's volume, omitting, from want of space, any notice of the concluding portion—the 'Statistics of the Retreat.' The value which our readers will attach to this work, will probably much depend on their views as to the importance of statistical investigation in general. For our own part, though fully alive to the cautions necessary in the application of numbers to the elucidations of questions of medical science, and to the fallacious conclusions which may be drawn from hasty, inaccurate, or too scanty observations, we believe that there are numerous points regarding the natural history of diseases, and the various causes which may modify their symptoms, progress, and results, and the effects of remedial agents, which can only be satisfactorily solved by the employment of the numerical method. This we think peculiarly the case as regards insanity, and we may appeal to Dr. Thurnam's work as an example of the interesting and novel results which may thus be deduced; and which, though they might be suggested by ordinary individual experience, are only capable of being established as facts, by calculations founded on a large number of observations. Those only who, like ourselves, have been somewhat extensively engaged in statistical researches, will be able fully to appreciate the amount of labour and care which, to ensure the accuracy which throughout characterises Dr. Thurnam's work, must have been required in its compilation; and we cannot but regret that, by adopting a more simple and lucid arrangement, and condensing his matter into a smaller space, he had not presented his observations in a more attractive form. To the medical officers of hospitals for the insane, and indeed to all those engaged in the cultivation of medical statistics, his work will afford much valuable information as to the methods to be adopted in the numerical registration of the results of their experience, and the errors to be avoided in generalizing from them. To such we believe the abstract we have given will be a sufficient recommendation. To the general professional reader, works of this kind must necessarily be less attractive than those of a more practical description, and we have hence been the more anxious to present as full an analysis of it as our limits would permit. We feel convinced, however, that those who are engaged in the cultivation of this branch of practical medicine will find sufficient in the work itself to reward them for its perusal.

ART. VII.

1. *Practical Surgery.* By ROBERT LISTON. Fourth edition.—*London*, 1846. 8vo, pp. 582.
2. *Lectures on the Operations of Surgery, and on Diseases and Accidents requiring Operations.* By ROBERT LISTON, Esq. F. R. S., &c.; with numerous Additions, by THOMAS D. MÜTTER, M. D., Professor of Surgery in Jefferson College, Philadelphia, &c.—*Philadelphia*, 1846. 8vo, pp. 566.

I. IN the Fifth Volume of our Review, p. 457, we gave a somewhat lengthened analysis of the first edition of Mr. Liston's 'Practical Surgery,' and extracted what appeared to us the most novel and interesting of his observations. The two next editions were allowed to pass almost unnoticed, their rapid succession being a sufficiently decided expression of the spirit in which the work was received by the profession, and amply bearing out the very favorable opinion we originally passed upon it.

The appearance of a fourth edition reminds us that opportunity has been given for the further development of the work; and, accordingly, after a careful perusal of the new volume, we propose to lay before our readers the chief additions to facts, and modifications of opinion, which we find therein contained. Such changes are looked for with interest, as the result of several years' additional experience to a surgeon who occupies a prominent and distinguished place in the ranks of his profession, and possesses large opportunities of pursuing practically the subject on which he writes.

The object, plan, and general arrangement of the work remain unchanged; the amount of information contained within its pages has, however, been increased; facts have accumulated, and the illustrations of particular forms of disease and methods of treatment have been enriched by the addition of several interesting and pertinent cases, and many useful and admirably executed woodcuts.

Of the object the author has in view we need say little, and it would, indeed, be equally idle to insist here on the importance of the manipulative branch of surgery. Mr. Liston has the credit of having perceived and supplied a defect in the surgical literature of the country. Since the publication of Mr. Liston's work the deficiency it supplied has been pretty generally acknowledged, and more than one rival treatise has started into existence, proving, we believe, the increased attention drawn to the subject, and the greater anxiety of the junior members of the profession to fit themselves for the management of cases occurring in country practice, which, in years gone by, were at once and as matter of course transferred to the hands of the London surgeon.

Mr. Liston's volume may be described as a series of essays on the principal injuries and diseases which require manual interference on the part of the surgeon, together with directions as to the modes of proceeding which he has himself found most convenient and successful. As a guide to the advanced student, and as suggesting practical observations of the highest value to the practitioner, it is unsurpassed; though, as a complete text-book of practical surgery for the younger student, it is capable of improvement.

Mr. Liston tells us, in his Preface, what is and what is not to be expected from his work:

> "The young surgeon ought, without doubt, to understand well the history of his profession, and this is only to be acquired by a perusal of the leading surgical works from the time of Ambrose Paré downwards to Wiseman and Heister. . . . . The reader of this work would not be much wiser, however, or more capable of undertaking the management of difficult cases, were the author to enter into a long detail of what this or the other of the moderns have recommended, or what he has found by experience in practice to be useless or hurtful. It has been the writer's aim to present a practical work, to which the young surgeon, when in difficulty, can refer, in hope of finding concisely stated and without conflicting opinions, that which he himself has, after a good deal of experience, seen and ascertained to be the wisest and safest course to pursue under various circumstances." (Preface, p. v.)

In the general spirit of these remarks we fully concur. We are no advocates of the practice, too prevalent in French Manuals, of cramming professedly practical works with detailed descriptions of the particular operation of this surgeon, the modification of that, or the proposal of another, for arriving at the same end. It may be well to put on record, and keep for reference, in dictionaries and encyclopædias, these numerous and often small modifications in practice, as proposed and carried into operation at various times and in different countries; and though practically many such propositions may have been long superseded by better modes of operation, they still form interesting records of the amount of intelect and attention turned to the subject. Thus collected, they will be consulted by all well-educated surgeons, and may serve sometimes as a means of humbling the pretensions of many a candidate, too eager for distinction, who would produce as a new discovery some exploded theory or practice. But to introduce such particulars into a work intended to guide a practitioner in the best management of particular cases, is to run the chance of producing confusion instead of giving confidence, and has the appearance, to say the least, of an ill-timed exhibition of learning. All this we are prepared to admit; yet we think Mr. Liston might have gone further into detail on some points: for although it would be a waste of time to detail every thing "he has found in practice to be useless or hurtful," it might often be worth while to give concisely his chief reasons for adopting and recommending this or that particular mode of procedure in preference to any other. The young practitioner, who consults the works of a practical man more in the hope of finding a decidedly expressed opinion to guide and strengthen his judgment, than with the view of refreshing his memory on the history of improvements in practice, will not consider the weight of the opinion lessened by a short statement of the grounds on which it is based.

We venture to throw out these suggestions because we are anxious that a work so worthy of its author should have its utility extended; and we are convinced that a brief statement, perhaps in the form of notes, of reasons which decided the operator in preferring one method to another, would be invaluable both to the practitioner and to the student.

We now pass on to a more detailed examination of the chapters which have received additions or undergone some change.

Appended to the first chapter, which treats *of the division of parts by the knife, and the means of suppressing hemorrhage*, we find remarks on the employment of astringents and escharotics, in which the author points out the preparations he has found by experience best adapted to fulfil certain special indications.

> "Some will be found to serve one purpose well, others another. In fact, as far as I can apprehend, after some experience, every caustic has its own peculiar properties and uses, and no one of them can supply the place of others satisfactorily. If an escharotic is plainly wanted, the suitable one must at once be brought into play; they cannot with propriety or advantage be used indiscrimately." (p. 26.)

This is a point which may seem small and unimportant, but it is one which we have reason to believe does not receive the attention it deserves. On the one hand, it is too much the habit of would-be surgeons to trifle with caustics, and to apply them as a matter of routine practice in every case where they find a sore halt in the process of cicatrization; and, on the other hand, it is comparatively of rare occurrence for a practitioner to be called upon to use an escharotic to considerable extent, yet with an exact precision. The uncertainty of the action of caustics is, with many, almost proverbial. In one instance the surgeon is obliged to repeat the application again and again, in order to produce an effect he hoped to attain by the first use of caustics; whilst, in another case, he is perhaps annoyed to find, when the sloughs separate, that the soft parts have been more widely and deeply destroyed than he intended or expected. These are accidents which must frequently come under the observation of those who have an opportunity of witnessing much practice, and we are inclined to infer from them, not that the escharotics themselves are so uncertain in their action, but that there is very frequently a want of anything like a fixed principle to guide in their selection and application. Mr. Liston finishes his observations on this head by the following characteristic advice:

> "In conclusion, if tissues are to be destroyed, let it be done at once, by the use of a proper and efficient caustic; and this is much better for all parties concerned than the practice of piddling from day to day with little or inefficient applications, as is often done in lupus and other diseases. The patient's patience is worn out, and the disease thus often deteriorated." (p. 28.)

In the chapter on *injuries of the bones* (part of which, as on fractures of the spine, for example, might with advantage be extended), we would draw attention to Mr. Liston's opinion on the importance of an early adjustment and elevated position in cases of fracture of the long bones. This he expresses in the following strong terms:

> "The object of the surgeon in the treatment of all fractures must be to obviate pain and suffering, to put the parts in the most favorable condition for being repaired, and to preserve the limb of its normal shape and length. All these indications are fulfilled by the same means, viz. instant coaptation, and retention of the broken ends in the most perfect possible apposition. The earlier the means are adopted, the greater and more immediate will be the patient's relief from suffering, and the less the surgeon's anxiety and labour; excess of bloody and serous effusion will thus also be prevented, and the excited action kept within bounds. Prevention, it is admitted, is always better than cure; and if the above recommendation is attended to, all necessity for local abstraction of blood, and for the use of lotions to cool the part, will be obviated. If, on the contrary, the limb is laid loosely on a pillow, in an easy position, as it is by some thought and said to be, muscles relaxed, &c., and no efficient means are employed to prevent the spasmodic action of the muscles, the startings of the limb, the jerkings of the broken ends, and the displacement of the fragments, then assuredly, in spite of all local and general measures, there will arise frightful swellings, pain, tension, and heat; the intermuscular tissue will be gorged with blood, and the circulation of the limb roused to a dangerous and alarming degree. No application of soap plaster, in strips, gently around the limb, 'by causing secretion underneath it,' can reasonably be supposed likely to diminish either the irritability of the muscles or the urgency of the inflammation; nor will it. The splints, when resorted to, must be put upon a swollen and distorted member." (p. 62.)

Experience and observation have already proved to us the propriety of this advice, and its soundness as a safe principle of treatment; though, in this as in every other case, particular exceptions may demand a deviation from the general rule. In country practice, for example, where possibly both patient and surgeon have to travel some distance before the injury can be attended to, where consequently a considerable interval is unavoidably allowed to elapse before the necessary materials for "putting up the fracture" can be collected, and where the habit and constitution of the patient render him more prone to the early occurrence of inflammatory action, the surgeon may sometimes find it advisable to resort, in the first instance, to fomentations, evaporating lotions, &c., the limb being elevated, however, and just so far confined as to prevent it from starting, or the ends of the bones from riding over each other. But in town practice, and generally in hospitals, where competent assistance is always at hand, and where the exact nature of the injury can probably at once be satisfactorily made out, we believe that the necessity for delay in the adjustment of the fracture and the application of splints is of extremely rare occurrence. It surprises one who has been in the habit of following this method to hear the surgeon of a metropolitan hospital recommend that a fracture should be left on a pillow for a few days, enveloped in strips of soap-plaster. We might ask what object is attained by the plaster that would not be equally well, or better, effected by the ordinary apparatus? A question of equally difficult solution is, what objection can be made to the application of splints and a moderately tight bandage, that does not equally apply to the employment of strips of plaster? The young surgeon who has been taught to expect the supervention of inflammatory symptoms as the natural consequence of breaking the bone, is rather confirmed in his erroneous impression by finding that, in spite of what he is led to believe the best precautions against it, inflammation sets in. It perhaps never occurs to him to suspect that the very precautions he observes may tend to produce that result. Let him, however, treat an equal number of cases on the principle of immediate adjustment and elevated position, and we feel convinced he will be surprised how seldom the evil he apprehends will actually manifest itself.

We must refer our readers to the work itself for much interesting matter in the chapter on *injuries and diseases of joints.* The indications of treatment are laid down clearly, and the subject is handled concisely and with strong common sense. One division of the chapter is headed "Resection of Bones," but the only operations described are those on the elbow and shoulder-joints. The methods of proceeding for the resection of the upper and lower jaws and clavicle are detailed when treating of tumours and morbid growths; and excision of the articular surfaces composing the knee and hip-joints, is not considered by Mr. Liston sufficiently encouraging in its results to demand any detailed notice from him. The value of the operation as applied to the elbow and shoulder-joints, is now generally admitted, though there is still a want of statistical information (more particularly as regards the arresting diseased action and preserving the after utility of the limb), a deficiency which can be supplied only by following up the history of persons so treated for several years after the operation. It would, however, appear to be established by facts, that the result of excision of the joints is most favorable in children; and this we think might have been expected. Nature at this early period of life has more power to repair extensive injuries, and to make up for deficiencies produced either by the ravages of disease, or by the means taken by the surgeon to arrest its progress. The material by which the uniting of the several ends of the bones is effected, after excision of the joint of a child, is more elastic and pliable, and forms a better substitute for a capsular ligament than the tough and irregular deposit that forms the medium of connexion in the limbs of older persons. The development of the parts, the growth of particular muscles, and the habits of the patient in using and managing his limb, are all probably more easily and thoroughly modified in the instance of a young person. Even in an adult, however, under favorable circumstances, a very serviceable limb may be the result of the operation; and with a careful selection of cases, there is every reason to expect that the operation will continue (as it has already done for the last few years) to rise in the estimation of practical surgeons. Happily, it is not so likely as some more brilliant operations to be too generally or indiscriminately employed. In the case of the elbow, for example, the method of excision is so much more tedious and painful, so much more difficult of execution, and so much more severe in appearance than amputation of the arm, that the greater number of surgeons naturally lean towards the latter alternative, without perhaps sufficiently considering that the patient may feel amply repaid for extra suffering in retaining his forearm and hand.

Any undue preference of this kind will be gradually corrected as our information on the subject accumulates, and in the meanwhile we believe that the operation of excision has a better chance of being appreciated than if, instead of being received with hesitation, it had at once been over-praised and indiscriminately adopted. It might be said that excision of the joints has retrograded in consequence of being originally thus overvalued, because this method of treatment remained for many years comparatively unnoticed, until again urged upon the attention of the profession by Mr. Syme. But we conceive that other causes have been in operation to produce a fairer estimate of excision; for, as now modified and limited in its application, that mode of treatment is already on a very different footing from that on which it was placed by Park and Moreau. Speaking of the circumstances under which the operation has the best chance of success, Mr. Liston remarks:

> "Such operations may be resorted to with propriety when the patient's general health has not suffered much, or when, from keeping the joint in a state of quietude, the discharge has abated, and the system recovered from the effects of the irritation and constant drain. When, however, the soft parts are much diseased, when, as is often the case, the disease is not limited to the articulating surfaces, or when the patient is reduced to a very low state by hectic, it may not be very prudent to try the experiment; for it has happened that, after all, the proceeding has not been followed by removal of the whole disease, or subsidence of the constitutional disturbance; and in the end amputation has been deemed advisable. And this latter operation, which alone and in the first instance, might have been borne up against, has at that stage been followed by a fatal result." (p. 152.)

Excision, as employed in the case of the elbow, is seen in its most favorable application. It should be borne in mind that this joint is, for many reasons, particularly well circumstanced to ensure success. As regards the operation, the elbow is so situated and constructed as to render comparatively easy the full exposure and safe removal of the diseased surfaces of bone; and in reference to the final result a very useful limb is preserved, even in cases where but little motion remains in the new joint.

In performing this operation Mr. Liston always adopts the same lines of incision. The first is made in the direction of the length of the limb, and close to the radial side of the course of the ulnar nerve; the second extends from the centre of this incision, across the joint, to the outer condyle, and the two flaps marked out are then dissected from the bones and turned upwards and downwards. The advice given by some writers, to expose the joint by slitting up sinuses where they exist, appears to us more plausible than sound. Such a proceeding saves the patient very little, if any, pain; it may seriously hamper and embarrass the surgeon in the subsequent steps of the operation; and, lastly, it is not absolutely required, because all these sinuses readily and rapidly close when the cause of irritation is removed, and when a more direct and free exit for the discharge of pus is established. The soft parts being reflected from the bones, Mr. Liston directs that the forearm should be flexed on the arm, and the ligaments at once freely divided. In this position he finds no difficulty in dislocating the bones, and examining the surfaces which enter into the formation of the joint, without first sawing off the end of the olecranon, as practised and recommended by Mr. Syme and others.

Cases are recorded of excision of the elbow, even in adults, in which the use of the forearm was retained in an extraordinary degree; and so far they would tend to encourage the early employment of passive movement of the limb after the operation. It is possible, however, in attempting too much, to lose all the benefit of the operation; and, considering the insufficient data we at present possess, it must be regarded as a delicate question how far the surgeon should aim at reestablishing the motions of flexion and extension of the forearm. On this point Mr. Liston appears to us to convey a wholesome caution, by not speaking in quite such sanguine terms as some writers have done:

> "Unless the operation on the elbow be performed when the patient is very young, and the portions of bone removed be very small, the motions of that joint must of necessity be ever after very weak and unsteady. In the operations which I have performed on the adult, amounting in all to eight, for thorough removal of the ends of all the three bones, I have aimed at bringing about a kind of ligamentous anchylosis, by steadying the part for a long time by means of leathern splints. In fact, the patient, in order to use the forearm and hand to any advantage, will require for a time at least, to have some permanent contrivance to fix the forearm at a right angle with the upper arm." (p. 152.)
>
> "In more favorable cases the patient will acquire very considerable power of motion in the false joint. He will be able to straighten and flex the arm almost as completely as before the operation. The union in these cases is not usually effected by anchylosis, but a dense fibrous tissue passes between the ends of the bones at all parts, and the whole is surrounded by strong ligamentous substance, to which the muscles are inseparably united. The fibrous tissue between the bones is longer or shorter as more or less motion is permitted." (p. 154.)

In Chapter VI, *On injuries and diseases of blood-vessels*, some interesting and instructive cases will be found of hemorrhage from ulcers and abscesses, produced by an extension of ulceration, from without inwards, through the coats of the larger vessels. Both arteries and veins are subject to this accident; but the latter class of vessels are more frequently so affected in the case of ulcers. Several instances, however, have been collected by Mr. Liston of perforation of the coats of arterial trunks and branches, to the extent of effecting a free communication with the sac of an acute or chronic abscess; and these have been noticed by us on a former occasion. (British and Foreign Medical Review, Vol. XV, p. 155.) The following case, which occurred in the practice of Mr. Cheyne, of Leith, is added:

> "H. R., æt. 6, Nov. 1828, was treated for chronic peritonitis. Two years before this attack he had suffered from fever, with pulmonary symptoms, for five weeks. After recovering from this disease, he enjoyed tolerable health till within four weeks of his death. He was then seized with remittent fever, but no region or organ appeared particularly affected till the end of the first week, when the belly rather suddenly became tympanitic, and the child complained much of pain on pressure. The remissions became more distinct; and during the fourth week a dry and rather severe cough troubled him. At the end of this week, one day

about noon, he vomited a large quantity of blood; rallied a little; but in an hour after, upon vomiting more blood, he rapidly sank. On dissection, the peritoneal surface bore all the marks of intense inflammatory action having existed; the membrane was much congested and thickened. In the stomach about a pound of clotted blood was found. At the upper part of the descending aorta there was a small ragged perforation, leading to a cavity that might contain a walnut; this cavity, filled with coagulated blood, communicated with the œsophagus by an orifice capable of admitting the little finger. It was situated in the midst of the bronchial glands, which were more or less changed into a texture of a cheesy appearance The lungs adhered pretty generally to the thorax by bands of old formation. In their substance there was a yellow tuberculous mass, of the size of a marble, and a few smaller ones in its vicinity." (p. 192.)

After describing the operations for applying a ligature to the external iliac and femoral arteries, Mr. Liston mentions the treatment of aneurism by pressure over the arterial trunk at some convenient point between the tumour and the heart, in the following terms:

"Within the last few years the treatment of aneurism of the middle of the thigh and ham by pressure, in a modified form, has been revived with very encouraging results. In all, some ten or a dozen cases, have up to this time (1845) been so managed. The practice was first employed by Mr. Hutton, and followed by some of the Dublin surgeons. A few cases have also been cured in this country; two of them under my care in the University College Hospital. The object in applying the pressure is not to stay entirely the current of blood, but merely to weaken it, so as to permit coagulation of the contents of the sac to proceed gradually until this process is completed. For this purpose the femoral artery has applied over it a pad secured by a sort of C-shaped piece of steel, jointed in the middle, and with a larger pad by which counter pressure behind is effected..... The skin over the vessel should be protected with plaster on the back of the thigh, by a well-fitted leather splint. The patient may have almost the entire control over the instrument, screwing it up or relaxing the pressure according to his feelings. It is by no means a bad plan to apply two such tourniquets, which can be tightened or relaxed alternately. By adopting this method a cure may pretty confidently be expected, and the patient is certainly saved from considerable risk, as from secondary hemorrhage and inflammation of the vein." (p. 222.)

The advantages of the treatment by pressure appear to us to be by no means limited to the avoidance of the dangers which are incurred by the application of a ligature in the usual manner; such as secondary hemorrhage from the separation of the ligature, and phlebitis. In the first place, the plan by pressure is one likely to be much more readily submitted to by patients than what is known to be a more severe and hazardous, and is believed (although erroneously), to be a more painful proceeding: and thus not only may many cases be brought under treatment that would otherwise be allowed to advance gradually to a fatal termitation, but many more may be attacked at an earlier period than is now permitted under the contemplation of a different alternative.

The general principles on which the treatment by pressure is now based are so simple, and the precautions necessary to be observed so few and easily understood, that we may state facility of application as another recommendation—not, indeed, to the accomplished operator, but to the more numerous class of surgeons who, with fitting cases for pressure under their charge, are unwilling to grapple with the difficulties of an operation which requires in the operator a practised hand and confidence in his knowledge of the anatomy of the parts concerned.

Again, the mode of treatment by pressure has this advantage; it is capable of being interrupted for a time, or entirely stopped, if necessary. On the other hand, when a ligature is applied on the main trunk of an artery the step is irretrievable; and if the arrest of the circulation through the limb should prove to be too complete, and the usual signs and symptoms of mortification should in a few hours begin to appear, there is little to be done but to await the more unequivocal indications of failure, with the alternative of mutilation, or almost certain death, should the symptoms increase. Lastly, there is every reason to regard pressure as so safe a proceeding, that it may be applied in instances where the attendant circumstances forbid a more hazardous course of treatment.

It is perhaps rather premature to affirm that, even should the treatment by pressure fail, the patient is left in no worse position than before the attempt. The number of cases at present on record is hardly sufficient to justify so unqualified an assertion. If the case be of such a description that an operation by ligature is considered unjustifiable, the failure of the plan by pressure to produce a cure certainly implies no aggravation of the evil; whilst so far as even a temporary arrest of the progress of the disease has been effected, the patient must be considered to have gained by the treatment. But if, on the contrary, the case be of a nature which offers, at first, a fair prospect of success to the operation by ligature, it is possible that long-continued compression may produce such an alteration in the condition of the main artery, and of the parts immediately surrounding it, between the seat of pressure and the tumour, as to throw difficulties in the way of the subsequent application of a ligature, which would not have existed had not pressure been resorted to. And this leads us to remark that in cases of the kind last referred to, it may be a principle worth holding in view to apply the pad of the instrument at such a point as to leave sufficient room above for the proper application of a ligature (should such a procedure be found, after all, advisable) to a portion of uncompressed artery.

One of the most interesting and valuable parts of the volume is that which is devoted to a consideration of *morbid growths and enlargements.* As the principal object is the description of the variously modified operations that are required for the removal of these growths from different situations, Mr. Liston classes tumours according to the localities in which they arise; and only touches incidentally upon their differences in structure and general character. Speaking of some of these diagnostic signs between simple and malignant tumours—between those he would remove and those he would leave untouched, Mr. Liston takes occasion to reprobate, in strong terms, the frequent and indiscriminate use of grooved needles and exploring trocars:

> "The surgeon, by studying well the nature and progress of growths in this situation (the upper jaw), and, in fact, in all parts of the body—by losing no opportunity of making examinations, and thus gaining the requisite tact, will be able to predict their internal structure with tolerable certainty. He will not be under the necessity of resorting to what, in many cases, proves a very mischievous and hurtful influence, the thrusting into their substance of a long, flat or round and grooved needle. I was consulted very lately by a gentleman labouring under a tumour occupying the floor of the antrum, who had shortly before suffered very severely in consequence of this idle curiosity; the action of the diseased part had

been roused, and for some time after considerable increase of bulk had taken place. A day or two after this there was presented at the hospital a case of tumour over the distal end of the ulna, of firm consistence and very painful nature, involving and compressing the dorsal branch of the ulnar nerve. There was cicatrix resulting from a punctured incision upon the skin covering it. The nature of the case was represented to the young woman, and extirpation recommended. She returned to have this done in two days, with a fresh mark of puncture upon the tumour. The operation was performed, and inquiry then made as to how this second mark had been made. She confessed that she had, in the interval, gone to another hospital for advice, had there the swelling explored, and suffered in consequence, as she declared, much more pain than that caused by the extirpation." (p. 314.)

Mr. Liston, however, admits the value of the information derived from this mode of examination, and countenances its employment in obscure cases:

"I had recourse to the practice lately in a very obscure case. A middle-aged woman was admitted into the North London Hospital, labouring under a large tumour situated in the lower part of the abdomen, and filling the pelvis. The bladder was displaced, and occasional retention of urine took place; difficulty was always experienced in introducing a catheter, and it was necessary to depress the handle so as to convey the point of the instrument close to the symphysis. Indistinct fluctuation, it was supposed, could be detected by pressing alternately on the tumour above the pubes, one finger being placed on the projection in the vagina, the other on the external swelling; the os tincæ could neither be felt nor seen by the use of the speculum. A large grooved needle was introduced into the vaginal tumour, and a small quantity of pus was perceived. This was immediately followed by a large flat trocar, and nearly three pints of well-digested pus evacuated. The patient ultimately died; the cyst was found connected with one of the ovaries, and contained, besides matter, a large ball of hair—no uncommon occurrence." (p. 315.)

We have not room for more than a passing reference to Mr. Liston's directions as to the various operations for the removal of tumours. The manner in which the whole subject is handled is well calculated to impress the reader with the conviction that the author is speaking from large personal experience and frequent trial of the particular modes of practice he recommends.

The first object in the performance of these irregular, and often extremely perplexing and difficult, operations is to make sufficiently free and extensive incisions through the skin, to ensure the complete exposure and most ready dissection of the chief attachments of the morbid growth; and this "good beginning" is all-important, not only because it really shortens the suffering of the patient, by giving every possible facility to the after steps of the operation, but because it enables the surgeon to satisfy himself more thoroughly as to the entire removal of the diseased tissues. The next object, and one often of no slight importance, is so to arrange these incisions as to leave as little disfigurement as possible, either from loss of integument or from unseemly and conspicuous scars.

These considerations seem constantly present in Mr. Liston's mind; and much ingenuity is displayed in his plans to carry out both objects. A reference to his directions for the removal of parts of the upper and lower jaws, on account of tumours affecting the bones, and ligature of erectile tumours not involving the skin, will sufficiently illustrate these remarks.

Nothing is said yet of the plan for the *subcutaneous* applications of ligatures, as lately proposed in Paris by MM. Ballard and Rigal, for the destruction of tumours in the thyroid gland, and applicable, it may be supposed, to cases of nævus. We can readily conceive, however, that the cases which would admit of this somewhat more complex proceeding are by no means numerous or common.

The chapter on *amputations* has received further illustration by several new and well-planned wood-cuts, but is otherwise little altered. Mr. Liston is well known to be a strong advocate for the method by flaps, and for leaving the dressing of the stump until all oozing of blood from the cut surfaces has ceased. In amputation of the leg he seems to prefer dividing the tibia and fibula to separating the limb at the ankle-joint; the latter operation, though lately so much recommended, is not described or noticed by him. Amputation at the elbow-joint is advised in cases of injury involving a great part of the forearm.

Several good cases will be found in the next chapter, illustrating the *lodgement of foreign bodies in the windpipe*, &c.; and a successful case of extraction of a button from the bronchus is also recorded. The patient, a boy eight years old, allowed the button to slip into his trachea whilst at play, and was instantly seized with extreme difficulty of breathing, threatening suffocation. On the tenth day after the accident, the irritation having in a great measure subsided, and simpler means having failed to dislodge the foreign body, the windpipe was opened by Mr. Dickin, of Middletown, the opening extending from the crico-thyroid membrane downwards for about an inch. A pair of long and slightly bent forceps being introduced, the foreign body was struck, and, on a second attempt, seized and extracted. Another case is related, in which a bullet was dislodged from the bronchus by the sudden inversion of the patient, who was strapped to a common chair and slung to the rafters of the ceiling. He was turned over three times and repeatedly shaken, and the third time the bullet rolled from his mouth.

Lithotrity. Though known to be a very skilful and successful lithotomist, Mr. Liston appears to have recognised and practised the operation of lithotrity at an early period; and, as far as we can judge from the terms in which he speaks of this mode of treatment, his confidence in its efficacy, within certain limits and under certain restrictions, is increased by additional experience, and by the improvements that have been effected in the construction of the crushing instruments. After shortly comparing the advantages of each plan under different circumstances, he adds, "of late years I have scarcely been obliged to have recourse to lithotomy at all in private practice." It is hardly necessary to remark that such an admission is of peculiar value, and comes with additional weight from a man who, with large experience and a high reputation as a lithotomist, may naturally be supposed to have a certain, even unconscious, leaning towards an operation in which he feels his strength, and of which the difficulties are universally acknowledged. Mr. Liston's estimate of the value of lithotomy and lithotrity is, we think, fairly and candidly stated. The accidents that may follow the operation of lithotrity, far from being overstated, might with advantage be dwelt upon more fully. These accidents—including inflammatory congestion of the parts around the neck of the bladder and membranous portion of the urethra, hemorrhage, cystitis, impaction of fragments in the neck of the bladder, and distressing spasmodic action of the parts in the neighbourhood, retention of urine from this latter cause, or from the presence of coagula in the bladder, or from partial paralysis of that viscus, arrest of pieces of calculus in the urethra, swelling of the testicle, and even suppression of urine—should all be fully considered, not only for the purpose of detailing the appropriate means of treatment, but with the view of fairly setting before the surgeon who undertakes lithotrity the accidents which he may meet with, and must be prepared to avoid or overcome. The two methods of operation, as witnessed in one or two ordinary and perhaps successful cases, leave very different impressions of the dangers to which the patient is exposed—impressions which, we may add, are quite disproportioned to the real risk incurred. French lithotritists have, with great power, drawn a vivid picture of the horrors of lithotomy, in order, apparently, to induce to the belief that lithotrity, because it does not present to the eye all those startling accompaniments, is without its dangers. But this is a short-sighted policy; even assuming that these supporters of lithotrity are thoroughly convinced that the measures they recommend are much more frequently applicable than is generally supposed. This disingenuousness, as is the case with every, even the slightest, exaggeration or suppression of facts, produces a twofold mischief. The immediate effect of inculcating such prejudices on an inexperienced mind may be to lower the estimation of lithotomy; but the ultimate effect (and one is sure to follow the other) is an equally unjust depreciation of lithotrity, when it is found unexpectedly that a surgeon may, in attempting to crush a stone, occasion a more serious mischief than perhaps at once appears. In order that strict justice should be done to lithotrity, no less than that the surgeon should be able to apply it judiciously and effectually, the unfavorable as well as the favorable side of the picture should be exposed, and the operator put on his guard as to the difficulties he may encounter.

The chapter on *hernia* we only refer to in order to point out a decided advance in opinion on the subject of dividing the stricture in cases of strangulated hernia, without opening the sac—a change of opinion which is based on the most satisfactory of all foundations, namely, actual experience after a fair and judicious trial. All who are aware of the frequent occurrence of peritonitis after the ordinary operation for strangulated hernia, and reflect on the causes chiefly concerned in such cases in the production of so unmanageable and fatal a disease, must admit as important and reasonable the proposal made by Petit, considerably more than a century ago, of avoiding, if possible, the exposure of the peritoneal surface. As limited in its application by Sir Astley Cooper, namely, as applied to old and large hernia, the principle is now recognised by most surgical authorities; but the further extension of the principle, as argued for by Petit and Monro, and more recently by Mr. Key and others, has met with strenuous opposition from some, and neglect from many more, who were content, perhaps on theoretical rather than on practical grounds, to throw doubts upon the soundness of the suggestion. First, the possibility of effecting the object, then the expediency of making the attempt, was called in question; and the result has been to delay the period at which, after a sufficiently extensive trial, an impartial judgment might be passed upon the practice. However, there are few improvements in medicine or surgery the expediency of which has been, from the first proposal, so manifest as to have escaped altogether a similar opposition. Indeed there may be said to exist a kind of professional *vis inertiæ* which can be overcome only by degrees, because the objections it raises must be met, not by argument, but by facts; and these require time to be collected in sufficient number.

Six years ago Mr. Liston expressed himself doubtfully, and, on the whole, unfavorably, of the plan of dividing the stricture without opening the sac. "It is," he remarked, "rarely possible to effect the object, although there is no harm done by trying it in some cases; but in others, constituting the vast majority, the attempt must prove very unsafe." Now, however, after a longer trial, he recommends the method more decidedly and unhesitatingly. After describing the preliminary incisions for exposing a strangulated femoral hernia, for example, he proceeds as follows:

> "If the surgeon aim at trying to give relief without opening the sac, as he always ought to do in cases of recent strangulation, he will now pass his narrow blunt-pointed bistoury betwixt it and the fascia, upon a director insinuated under the sharp and tight margin of the crural aperture (the director may often be dispensed with); then turning the edge forwards, slightly towards the mesial line, and raising its handle, he will divide the resisting fibres. He may now try to reduce the contents of the sac by pressure, taking especial care not to push back sac and all—rather a serious accident. . . . . . I have fortunately succeeded in effecting this object in a considerable number of instances within these few years; and it is a proceeding which I most strenuously advise the adoption of when nothing contraindicates it. The attempt can do no harm, it causes little or no delay; and if it is not successful, the sac, after all, is opened, and the operation completed. If it does prove successful, the surgeon's mind is relieved of an uncommon load of anxiety." (p. 557.)

In conclusion, it is scarcely necessary to repeat our earnest recommendation of Mr. Liston's work. Having on a former occasion expressed ourselves strongly on the subject, we can only add, that the present edition is, as it should be, even more worthy of our praise than its predecessors. The already practised surgeon will know how to value it; but the student and young practitioner stands more in need of a little warning to prevent him from forming a false estimate, not indeed of such works, but of his power to profit by them. The simple perusal of works of this high order, unless the student of surgery is careful, at the same time, to test his dexterity and correct his ideas by frequent and repeated trial on the dead body, will assuredly only prepare disappointment for himself and others. We are led to make this remark from having observed a tone which, more or less, pervades the whole of Mr. Liston's work, namely, that of rather under-estimating the difficulties of the measures he recommends. In one sense it would be unfair and unjust to assert that Mr. Liston's book is calculated to encourage an undue love of operating. A reference to his observations on excision of ovarian cysts, operations on cleft palate, division of muscles in cases of deformity of the spine, removal of malignant tumours, and many plastic operations, will satisfy the candid reader that he speaks much less sanguinely of the results of surgical interference in what may be called doubtful cases, than many writers on operative surgery. There is, however, a difference between sanctioning the attempt at interference in cases that ought not to be meddled with, and affording to ill-qualified persons a certain encouragement to undertake a method of treatment which, though in itself perfectly justifiable, they are not prepared to carry out. The terms "easy" and "difficult," as applied to different manipulations, may, when measured by the writer's own standard, be perfectly justifiable, and yet may mislead the inexperienced reader, and tend to engender in him a wish to operate, founded on a false confidence in his own judgment and skill. We believe that there are many, and we fear the tone we deprecate is likely to increase that number, who, when they know the kind of operation called for in a particular case, and can repeat methodically the different steps of the proceeding, feel themselves competent to undertake it. To these we can only say, in the words of our universal authority, "If to do were as easy as to know what were good to do, chapels had been churches, and poor men's cottages princes' palaces."

II. The second of the two works, the titles of which stand at the head of the present article, is a republication, "with numerous additions," by Professor Mütter, of Philadelphia, of some nineteen Lectures delivered by Mr. Liston, at University College, London, and reported in the 'Lancet,' during the years 1844 and 1845.

We have already said enough to convey an idea of the respect with which we regard Mr. Liston as an authority on the subject of operative surgery.

These Lectures, collected from the above source, embrace indeed many of the topics just noticed in his treatise on 'Practical Surgery;' they are treated here with much spirit; are illustrated by cases in the lecturer's hospital practice, which must have been under the observation of many of his hearers: and are occasionally enlivened with a somewhat freer discussion than he allows himself in his more formal work, of the lines of practice adopted and recommended by other surgeons. The style is characteristic, forcible, and well adapted to excite interest and arrest attention. Several points are only slightly touched on, or are passed over altogether; some of these are taken up by Professor Mütter, who thus adds [within brackets] about two hundred and fifty pages to the general body of the work.

Except typographically, we cannot say that these two elements are dexterously blended. Some of the additions are themselves excellent and highly practical; but, taken together, there is a want of harmony with the Lectures themselves, not only in style (which might be expected), but in tone and spirit, in arrangement, and in the mode of carrying out the plan. Though, of course, the result of much reading as well as of actual practice, the lectures of Mr. Liston are stamped with a certain individuality; the lecturer so contriving to look at and weigh even the discoveries of others, through the medium and by the scale of his own experience, that the whole of the information imparted comes with much of the freshness of originality—a quality which does not generally characterize the additions of Dr. Mütter. The frequent recurrence of phrases, such as the following: —"The great majority are in favour of as a general rule;"—"Most surgeons prefer;"—"The weight of authority is in favour of;"—"It is considered best;"—or, such and such circumstances "render this operation a favorite with most surgeons;"—give a character of feebleness to the writing, which no one who is acquainted with Dr. Mütter's high reputation as an operating surgeon, would think in any way applicable to the writer. Nor is this effect lessened by the detail of measures which are afterwards unceremoniously dismissed with the remark, that the proposal is merely mentioned "as one of the novelties of the day, and wholly unworthy of our confidence;" or again, that "no surgeon of any note would condescend to employ so puerile a means;" or, "it will soon be remembered merely as one of the idle whims of some inventive genius."

The Lectures are rapid sketches of the principal points of interest in a wide field; many of the additions are representations, begun on a more elaborate plan, but hurriedly finished, and less striking in the general result. Mr. Liston, it may be observed, presupposes a certain amount of knowledge of the general principles of surgery, as already given at a former part of the course by his colleague, Professor Cooper; and to this previous instruction he takes occasion to refer from time to time, when he passes lightly over abstract points, and adheres more closely to subjects of a strictly practical nature. Dr. Mütter proceeds on a different plan, and divides his subjects elaborately, as if giving the outline merely of a complete treatise. We can conceive two methods of oral instruction on such a science as surgery, each useful in its way, and yet each on such a perfectly distinct plan as to make it difficult (and only possible in an extended series of lectures) to unite the benefits of both. According to one plan, the lectures might consist of clear and graphic sketches of disease and its appropriate treatment, such as it exists in what may be called its typical forms, and always so connected with the relation of actual cases as to arrest and fix attention by rousing the hearer's sympathy. According to another plan, lectures might be made use of, less as a means of imparting particulars and minutiæ, than as an opportunity of directing the mind of the student in the exercise of thought; of pointing out to him the best and most credible sources whence to derive facts on the several subjects which present themselves; of giving him the valuable habit of arranging his knowledge in the best and closest possible manner, and of helping him to analyse, and to form a just estimate of the comparative value of information derived from different sources. These two objects might, indeed, be borne in mind at the same time, and carried out together by one lecturer, step by step; though we doubt the possibility of its being done in an ordinary course, in which a vast quantity and variety of matter is crowded. It would be a work of still greater labour to blend well together the results of two different minds directed to such different ends. We believe that Professor Mütter, therefore, was in error in wishing partially to combine elements of so different a nature and so difficult of combination; and in attempting to make these lectures (what they were never, to all appearance, intended to be) a complete and independent work.

The most important and interesting of the additions made by Dr. Mütter are those on the subject of plastic and other operations for the remedy of deformities and deficiencies, both congenital and acquired, of the soft parts. As the arrangement followed by Mr. Liston is based merely on the locality of the surgical diseases of which he treats, these observations and details of special operations are necessarily scattered through the work. This branch of operative surgery has been cultivated most perseveringly and with great success, by several surgeons in America, amongst whom Professor Mütter occupies a prominent position. His contributions to science, on this head more especially, are as important to the profession as we learn his practice has been creditable to himself and beneficial to those who have fallen under his hands.

The principle most clearly established by the repeated and varied trials of plastic operations appears to be that of making as broad and even a pedicle as possible to the transposed flap of skin, in order to leave an efficient vascular and nervous communication between the disturbed and the comparatively non-disturbed parts. The method of twisting the flap upon itself, (as in the Indian operation for the formation of a new nose) has, we believe, been adhered to longer and carried out further in this country than the success attendant on it has warranted. A much more successful plan (as Dr. Mütter shows from his own experience and from that of other surgeons) consists in cutting the flap in such a situation, that the direction it is made to assume is inclined at only a small angle from that of the surface from which it is taken. The pedicle need not then be twisted, and may be made broader than when this is requisite; or, still better, where it is practicable, separating the deep attachment of the skin, for some distance on either side of the cicatrix or part to be filled up, sliding the then moveable integument over the subjacent parts until the defective surface is completely covered; and, lastly, securing it in its new position by the usual means. As an illustration of this principle, we may follow Professor Mütter in some of his descriptions of plastic operations.

In the fifth Lecture, when treating of the mode of partially restoring the alæ of the nose, we find the following account of an operation performed by the Professor in the case of a gentleman who had lost "the whole of the right ala, as well as the adjacent soft parts, as high up as the os nasi of the same side," leaving a transversely oval opening, which measured three quarters of an inch across and half an inch in its vertical diameter.

"Seating myself in front I commenced the operation by making, with a small convex-edged bistoury, an incision extending from a few lines above the *superior* border of the orifice, to a short distance *below* its inferior, and directed *downwards* and *outwards*. It did not penetrate to the bone, but was sufficiently profound to allow a flap about *three* lines in thickness to be readily detached. This incision was completely on the *outside* of the cicatrix, a portion of which was subsequently removed, in order to prevent its hardened edges from irritating the raw surface of the flap, which was to be placed immediately upon it. One or two arteries were cut across, but the hemorrhage from them was arrested by pressure, until the *second* incision was made. This commenced at the *terminal* extremity of the first, *and extended horizontally outwards about an inch.* A *triangular* flap was thus marked out, and immediately detached from the subjacent bone by dissecting with the edge of the knife, held nearly parallel to the surface of the cheek. In the execution of this part of the operation two or three arteries of some size were necessarily cut across, and required the application of the ligature. The *third* incision, which extended from the *initial* extremity of the *first* to the point of the nose, was made with a pair of strong scissors, those being preferred to the scalpel, in consequence of this margin of the orifice being, to a certain extent, loose and unsupported. The triangular piece of cicatrix included between the superior ex-

> tremities of the first and third incisions, was then removed with the scalpel and forceps, and the sharp margin of the inferior portion of the opening also pared off, for reasons already stated. The hemorrhage having been arrested, and the parts properly sponged, the next step of the operation was undertaken. This consisted in the approximation of the first and third incisions, and the application of such measures as were calculated to retain the flap in its proper position. From the free dissection and the yielding character of the subcutaneous cellular tissue of the cheek, no difficulty was experienced in placing the edges in contact; and in order to ensure their perfect and close approximation, *four* stitches of the *interrupted* suture, made with saddler's silk, waxed and doubled, were passed. In addition, two or three small adhesive strips were applied to the spaces between the sutures. Finally, in order to prevent adhesion between the *flap* and the raw surface beneath it, and to give a better shape to the former, a small roll of soft lint, well oiled, was introduced into the *new nostril*." (p. 173.)

The sutures were not removed until the end of the fourth day, when union by the first intention was found to have taken place throughout. About the third week the lower margin of the flap was cut with a pair of scissors to give "the proper curve" to the ala. In consequence also of the septum of the nose being a little drawn to one side, apparently by the contraction of the flap, "the line of union between the base of the flap and the cheek was divided, cutting from within with a small scalpel, held parallel to the surface of the cheek, to the extent of three or four lines." The plug of lint also was increased in size, and a strip of plaster carried across from the tip of the nose over the sound cheek, so as to draw the whole organ in the opposite direction to that towards which it was inclined.

> "At the end of the sixth week from the day of the first operation, it was determined to execute the 'third step' in the treatment. This consisted in the division of the skin and cellular tissue at the base of the flap, in a *semicircular* direction, the convexity of the curve looking outwards. The object of this incision was to give the peculiar rounded margin of an original ala, to diminish the fulness of the cheek where the natural depression should exist, which depression had of necessity been destroyed by the tension of the flap, and to permit a return of the perpendicular position of the deviated septum nasi. The incision was made with a small scalpel, and extended to the bone. In order to prevent the union of its margins, a small roll of oiled lint was introduced into the cut, and a strip of adhesive plaster applied to the tip of the nose, and fastened on the cheek of the sound side. . . . . . On the third day the dressings were removed, and it was found that the margins of the incisions were nearly cicatrized and beautifully rounded off. . . . . . This latter change was a very favourable circumstance, as it produced a depression in the exact spot at which it was required, in order to give a proper expression to the face. Had it not taken place there would have remained a *sort of inclined plane* from the bridge of the nose to the outer portion of the cheek. At the expiration of the ninth week my patient returned home with scarcely a vestige of his deformity remaining." (p. 176.)

Truly, American surgeons have very far outstripped their English brethren when they can take into account the "proper expression" of the face. We can, from this, better understand the contempt with which another of their surgical writers, remarking on the subject, designates "the fruits of the dexterous manipulations of most nose-makers" as being, "most frequently, little else than pug-shaped, flabby, and moveable knobs, that look more like small shrivelled potatoes than *bonâ fide* human noses."

Under the head of cleft-palate, concerning the operations for the relief of which Mr. Liston speaks very discouragingly, Dr. Mütter introduces some admirable, short, and practical observations on the chief sources of difficulty and danger, both at the time of and after the operation; together with a brief detail of the means available for obviating the effects of such untoward and often perplexing circumstances. Dr. Mütter lays great stress on the advantage of simplicity in the form and construction of instruments to be used in these operations, and on the expediency of frequently handling the parts, for some days before operating, so as to "familiarise" the fauces to the presence of foreign bodies.

In the eighth Lecture is introduced an interesting account of a case of cancer of the lower lip, in which Professor Mütter removed the diseased parts, and at the same time restored the lip by a plastic operation. The latter step, which consisted in detaching and drawing up from the chin two symmetrical side-flaps, which were then united along the middle line, was ingeniously planned, and appears to have succeeded admirably. The patient remained well when seen after an interval of two years. We doubt, however, whether surgeons in this country would not have been satisfied, in the first instance, with getting rid of the disease, and deferring any further proceeding until the tissues near the excised part should have cicatrised, and remained for some considerable period free from any appearance of return of the disease.

The treatment of deformities resulting from burns is also much illustrated in this volume by Dr. Mütter. The author, as is well known, has published, in a separate form, several valuable essays on this subject; but as the principal of these were noticed by us in a recent number (Vol. XXXVIII, p. 396), we must refer to our former article for an account of the improved mode of practice recommended by him.

Dr. Mütter agrees with Mr. Liston in strongly reprobating the performance of "ovariotomy" in the present state of our knowledge of the diagnostic signs of ovarian dropsy. He here takes occasion also to introduce part of one of his own lectures, in which this opinion is expressed, with the candid and prudent reservation, "that should the difficulties about to be stated ever, by subsequent observation and research, be removed," he will be ready at once to change his present views, and rank himself among the advocates of the operation. Then follows a good summary of the most important objections that have been urged against the proposal.

Large, and we are inclined to think disproportionate, additions are made on the subjects of hemorrhage, cataract, anchylosis, club-foot, and division of tendons, which our limited space obliges us to refrain from noticing further.

Dr. Mütter's volume ends very abruptly. By way of completing the last subject (the treatment of calculous complaints), Mr. James Arnott is laid under contribution; and an account of the merits and principles of lithectasy, borrowed from that surgeon's paper, occupies rather more than ten pages. Unfortunately, Mr. Liston is not heard to the end; for his concluding lecture on lithotomy in the male, stone in the urethra, and on calculous and other diseases in the genito-urinary organs of the female is not here published. On referring to the "Lancet," we find that this lecture was reported in two parts, of which one appeared November 15th, the other December 13th, 1845; but neither, we suppose, reached Philadelphia in time for insertion in this volume. In his preface, dated January, 1846, Dr. Mütter says, "It will be observed that this volume contains all the lectures published up to the present date, but does not conclude the course. Another volume will be issued hereafter, should the publication of the lectures be continued." We should not have remarked on this little fact, had it not been a part only of the evidence the work affords of being composed with ill-judged haste; a haste which clearly originated in an anxiety on the part of the publisher to secure the profits of a promising speculation,—making it, as it would seem, quite a secondary consideration whether or not the work should be as worthy as possible of the two distinguished names under whose authority it appears. For this we do not conceive, nor would we for one moment be understood to imply, that the learned Professor of Jefferson College is answerable; but we cannot conscientiously acquit him of all responsibility. In default of a better and juster code of international law for protecting the fair interests and vested rights of authors (years of thought and study vested in print and paper), and defending them from the privateering system of the trade, we should like to see men of Dr. Mütter's character and position in the profession and in society, take their stand on the highest ground, and decline to append their names to hurried productions such as the present undoubtedly is. Nor is it expecting too much from them to do so, for lookers-on can see evident proofs of the ill effect the present system has upon their writings and upon themselves.

ART. VIII.

Die Acute Entzündung der Serösen Häute der Gehirns und Rückenmarks. Nach eigenen Beobachtungen am Krankenbett geschrieben. Von Dr. JOSEPH NEISSER, Prakt. Arzt zu Berlin.—*Berlin*, 1845.

Acute Inflammation of the Serous Membranes of the Brain and Spinal Cord; described from Personal Observation at the Bedside. By Dr. JOSEPH NEISSER, Physician at Berlin.—*Berlin*, 1845. 8vo, pp. 454.

AN apology to our readers is, we think, scarcely necessary for introducing to them a practical work on inflammatory affections of the brain. A correct diagnosis and treatment have an important social as well as a technical value. Insanity is considered by many to be a greater evil than death; imprisonment in a lunatics' hospital more distressing than entombment. We are convinced that a more correct diagnosis and treatment in this class of cases, would not only prevent the supervention of that form of chronic cerebral disease which constitutes insanity, but would obviate the serious mistake sometimes made by practitioners in the removal of cases of acute arachnitis to lunatic asylums as cases of mania. We know of more than one family on which the stigma of hereditary tendency to insanity has thus been fixed, and, we believe, we know also cases which have been maltreated in consequence of this mistake.

Dr. Neisser's work is divided into three parts: the first part, termed prolegomena, is a general consideration of the pathology of serous inflammation; the second is a detailed account of seven cases of cerebral and spinal disease; the third (the epilegomena) is an essay on the pathology and therapeutics of acute affections of the cerebro-spinal axis.

The history of these cases is, we think, peculiarly instructive. The symptoms as they arose from day to day are considered in their pathological, physiological, and therapeutical relations; the prognosis, diagnosis, and treatment are carefully deduced from a detailed review of previous experience or empirical knowledge, and from physiological data; and the whole forms both a useful contribution to medical art and a valuable criticism on medical science. It is, in short, an excellent monograph on the class of diseases investigated and treated by the author.

The serous membranes have little physiological interest; their function is only of a secondary character; but when attacked by disease they become of immense importance from the great influence they exercise over the contained organ. Inflammations of the serous membranes within the thorax, for example, are amongst the most common and, at the same time the most dangerous diseases; they are also much more frequently the seat of inflammation than the subjacent organs, if we except the lungs; they are justly considered by the practitioner as the most important class of inflammatory affections that he has to treat; for, independently of the primary diseases in organs to which they give rise, they are fruitful in secondary or sympathetic affections.

The structure of serous membranes is compound: they consist of a substratum of cellular membrane, well supplied with blood-vessels, and an unvascular epithelium. The substratum of cellular membrane is made up of several layers of fibrous tissue, the fibres of which cross each other, and have a regular arrangement. Whenever the vessels of the substratum are congested, the unvascular epithelium is raised, and projects into the cavity which it invests. The superficial distribution of the vessels of serous membranes renders the latter liable to rapid inflammation when the former are congested; and facilitates effusion through the parietes of the former into the serous sac.

The external surface of a serous membrane is attached to the organs or muscles external to the sac by the subserous cellular tissue; its anatomical peculiarities differ according to the anatomical relations of the true serous membranes, and the peculiarities give rise to characteristic pathological differences. The more plentifully the parts to which this subserous cellular tissue is attached are supplied with blood-vessels, the more is the serous membrane itself predisposed to inflammation, on account of the free anastomosis which takes place between the vessels of the contiguous structures. Thus the pulmonary pleura exhibits the most intense inflammatory action at the roots of the lungs, where the great vessels and bronchi enter; and at the fissure between the pulmonary lobes, where there is a layer of loose vascular tissue beneath the pleura. For the same reason, inflammation of the arachnoid membrane is more frequent on that side in connexion with the vascular pia mater than on that next to the dura mater.

A somewhat similar anatomical relation to the connecting subserous cellular tissue determines whether the inflammation be limited to the membranes or be extended to the organ it invests. Pulmonary pleuritis is rarely confined to the pulmonary pleura, and generally implicates the lungs, because its subserous cellular tissue is in immediate relation with the cellular tissue which connects the air-vesicles. Inflammation of serous membranes in direct connexion with fibrous tissues less frequently implicates the subjacent organs, because although the fibrous tissue is formed out of cellular membrane, it is comparatively less vascular.

The first stages of serous inflammation are the same as in other organs. Irritation being applied, the capillaries first contract, and so retard the circulation through them; then they are dilated and congested. Consequent on this is an effusion of serum through the parietes of the vessels, and the first result of serous inflammation is produced. The unyielding nature of the membrane itself facilitates the effusion as the dilatation of the capillaries is thereby restricted. If the inflammation ceases spontaneously, or is checked by art, the effused serum is readily taken up by the lymphatics; but if the inflammatory action continues, or increases in intensity, the blood-globules are heaped up in the capillaries, lose their colouring matter, and change their form; the blood then begins to coagulate within the capillaries; the lymphatics are no longer capable of performing their office, and the first stage of the disease, or the stage of inflammatory exudation, is accomplished.

The character of the exudation differs, however, with the progress of the disease. At first, when the capillaries are but slightly dilated, it is only watery serum; then plastic lymph or fibrin is effused, then pus; for the coats of the vessels become thinner and more porous as distension of them increases.

The products of serous inflammation differ in amount and character, from various circumstances which we need not enumerate, as these are well stated by systematic writers, and no doubt well known to our readers. False membranes are much less usual in the cerebral arachnoid sac than in the pleural or peritoneal; they are seldom, if ever, seen as a result of spinal arachnitis. Here the plastic power is at its minimum; in the serous membrane covering the heart and lining the pericardium it is at its maximum; the two being often strongly and intimately united by organized fibrin. The most remarkable exudation is that in which the colouring matter of the blood is effused, and sometimes even the blood itself. This variety is rare; it has been observed in inflammation of all serous membranes, but most frequently in that of the pericardium. Its true pathology is not yet expounded; but it indicates an intense and rapidly fatal disease.

Dr. Neisser discusses the pathology of serous inflammation as elucidated by microscopic researches, and especially the pathology of false membranes. The question of their formation is intimately connected with the physiology of development, and especially with the new doctrine of development by cells. There can be no doubt that researches of this kind will lead to new views both in pathology and treatment. What is the reason that the false membranes on mucous surfaces, as, for example, those formed in croup and in dysentery, never exhibit traces of organization? Why is the tendency to the formation of organized membranes so great in pericarditis, so insignificant in arachnitis? Dr. Neisser also takes occasion to discuss the question whether there be really a vis medicatrix, and resolves it in the negative: the processes developed in disease differ in no respect from those by virtue of which the embryo is developed and the tissues repaired. In reality, these processes lead to death; as, for example, in pericarditis and pleuritis, in fatal cases of which we often find the so-called healing process has established a close and intimate union between the contiguous membranes; constituting in fact one of the most incurable results of serous inflammation.

Dr. Neisser discusses at length the semeiology, etiology, and complications of serous inflammation: he is evidently intimately acquainted with this theme, and makes many interesting remarks. We do not, however, propose to enter into this subject at present, and we shall therefore proceed at once to a notice of the cases. The first shall be introduced in our author's own words as an example of clinical inquiry.

"FIRST CASE. *Thursday*, 16 . 7 . 40 [*July* 16*th*, 1840]. Gotthold, of young and vigorous constitution, aged 20 years, was received yesterday into the inner division of the Charité hospital at Berlin, in an unconscious state. We have no further information respecting him or his previous condition. The patient lies on his back, his head turned towards the right side, with closed insensible eyes, very red and turgid cheeks, and almost altogether unconscious. Respiration unequal and irregular,—a couple of short and superficial expirations, then a deep sighing inspiration; considerable feverish heat; quick and full pulse—104 powerful strokes. (?)

"*Hemorrhagia cerebri, apoplexia sanguinea, complicated with inflammation and fever.*

"I. *Exegesis of the symptoms;* absolute significance of the symptoms of the functional anomalies considered alone. The first glance at the morbid condition leads to an indefinite opinion; the seat of the disease is within the cranium. The untaught readily distinguish cerebral diseases; he lies helpless there, his eyes closed, &c. We approach him, and investigate his condition; he mutters, but scarcely moves. There is sensual and mental disturbance.

"The disturbance of the intellect presents a passive form; he is insensible, and appears neither to hear nor see; no loud delirium, no excitement; no violent movements. Left to himself he remains apathetic; he speaks not, nor does he move. If questions be addressed to him, he certainly exhibits some tokens of consciousness, but these consist in a stammering of unintelligible words, which only demonstrate his delirium. There is a lively agitated movement of the right hand and foot to be seen occasionally, but the left extremities are never used. That is very significant; in fact, when the left hand is raised up, it falls mechanically as if by its own weight. If the *left* arm is pinched, he moves the right hand as if to prevent it. There is clearly also hemiplegia of the left side, and (as often happens) sensation is less impaired than motion. No reflex (involuntary) movements are excited by touching or pinching the affected limbs. The temperature is equally increased on both sides of the body. The countenance is unaffected, is not drawn. The eyes when opened are in the same axis, but insensible to light; the pupils are unaffected. No deviation of the point of the tongue, either to the right or left. He does not swallow what is given him to drink, —a circumstance indicative of a dangerous degree of seizure.

"The respiration is affected in a remarkable manner; at the first glance we hear a slight sighing and groaning of the patient, as if from great grief.

"On a more minute examination we find it as follows: the patient makes 1, 2, 3 respirations, which follow each other at equal intervals, but are quite superficial; we see only the first pair of ribs slowly rise and fall. Then, however, there occurs a full, deep inspiration, with distinct expansion of the thorax, and very often accompanied by a sigh. Again 1, 2, 3, 4 superficial inspirations, interrupted by a deep and loud one, and so on. At the same time, it may be observed, that the left half of the thorax remains behind the right, and that the movements are effected almost altogether by the latter. Further, respiration inæqualis, unequal in the two halves; very abnormal in rhythm (respiratio irregularis), inasmuch as several superficial acts of breathing follow each (resp. sublimis, superficialis), then one very different, a deep full inspiration; resp. profunda, and almost suspiriosa besides. On the whole, the interval of time between two inspirations is rather prolonged, so that the respiration is not frequent.

"This manner of breathing is distinct from other anomalous modes of respiration, is easily distinguished from them, and is always easily recognized. It is so characteristic, that it is known in semeiology as respiratio cephalica, a symptomatic individualized form and indication, to be known at once without hesitation. It is *a sign of diagnostic value.*" (pp. 65-7.)

We have given the preceding as an example of our author's style. It is deficient in conciseness; it is in fact conversational. This, however, we conceive to be rather an advantage than a defect, because it detains the current of ideas upon each symptom, and renders the impression on the mind more distinct and lasting. As our pages do not admit of diffuseness, we must compress the author's views and strip them of their wordiness.

This *respiratio cephalica* is in truth an important symptom in diagnosis. It is sometimes absent, it is true, but is generally found to accompany pressure on the brain from whatever cause. It is pathognomonic of pressure on the motor centres, whether induced by serous or purulent exudation, hemorrhage, heterologous formations, as fungus, tubercles, &c., or direct injury to the skull, causing depression. If observed in children suffering from arachnitis, it is proof that exudation has already begun and advanced.

Special diagnostic synthesis of the affection from empirical pathology. Nothing is known of the previous history of the patient; no one can say anything about him, and he is unable to speak. It is true he is suffering from an affection of the brain, but what is the precise nature of the affection? It must have come on suddenly, and not long ago; his turgid, uncollapsed face shows that.

If it had come on at an earlier period, he would have been brought sooner to the hospital. It may be supposed to be apoplexia sanguinea, for the sudden hemiplegia, the loss of consciousness, and the character of the respiration point to this affection very distinctly. Opposed to this view, we have the increased temperature and the febrile pulse. Apoplexy from hemorrhage is characterized by a slow pulse and diminished temperature. If it be apoplexy there must be some complication, and the position of the head is in this respect a striking symptom. An unconscious apoplectic patient lying supine, never holds his head in a constrained position. And on examining the muscles of the neck they are found to be tense, swollen, and so tender to the touch, that if but slightly pressed the patient's countenance expresses pain, and he utters loudly some unintelligible words. Now rheumatism of the nuchal muscles not unfrequently extends to the vertebræ, and even to the spinal arachnoid membrane, inducing inflammation and its sequelæ. This, then, may be a cause of the accompanying fever.

In cases of apoplexy there is a stage in which the pulse becomes quickened and febrile symptoms arise; namely, the stage of softening. But these occur in from five to ten days after the attack, and the pulse is small and weak, as well as frequent. The face is also collapsed. We cannot infer softening, therefore, in this case. Sometimes a primary and sudden softening of the nervous centres is accompanied by hemiplegia; but it occurs either in very young or very old subjects, and principally the latter. There is also more consciousness than in this case, and the countenance is pale and sunken, the pulse weak.

As there is, therefore, cerebral hemorrhage, in what part of the brain has it occurred? Opinions differ widely as to the functions of the different parts of the brain, and as to the result of injury to different portions. Serres, Bouillaud, Foville, and others, have laid it down as a theoretical principle that injury of the thalami induces paralysis of the upper extremities; injury of the corpora striata paralysis of the lower extremities. As these parts decussate, the seat of the extravasation in the case before us must be referred (if we adopt Serres' views) to the right thalamus and its contiguous corpus striatum. The numerical method has shown that in about half the cases of apoplexy, the thalamus first, and then, with an increasing hemorrhage, the corpus striatum and the parietes of the ventricles are implicated. Hemorrhage on the surface of the brain is very rare in adults; hemorrhage at the base of the brain determines that rapidly fatal and incurable form termed by French writers apoplexie foudroyante. It is inferred, then, that the seat of the hemorrhage is at the points indicated.

Physiological and practical analysis of the symptoms, with reference to the seat of the hemorrhage. Having derived from experience, or empirical pathology, what information we can, it remains to examine the symptoms in their physiological relations. The physiology of the brain is so imperfect, and the views regarding its structure, relations to the nerves, and the functions of its various parts, so discordant and contradictory, that little can be done by this method. We will follow our author, however, through his attempted analysis; and, firstly, we have to consider the mental disturbance—the suspension of consciousness. Since the gray substance of the hemispheres is considered as the seat of the mental functions, we have in this case an impression radiated from the corpus striatum and thalamus to the hemispheres. Secondly, the activity of the senses is abolished. This may depend upon the loss of power in the hemispheres themselves; but we have proof, in the insensibility of the pupils to light, that the function of the visual centre is affected, for the reflex action through the ciliary nerves is interrupted. This fact does not, however, point exclusively to the thalami as the seat of the extravasation, because in affections of the corpora quadrigemina and geniculata, and other deep portions of the hemispheres, these seem connected with the sense of vision. It is also certain that this insensibility of the pupils is sometimes sympathetic, and the result of an injurious influence radiating on the visual centres.

Thirdly, the nerves, both of motion and sensation, are affected, though in different degrees. Indeed as all nerves, whether sensitive, motor, or sympathetic, end in the brain, we may expect that all will be affected in diseases of the brain, as in fact they are. We will take the nerves in order. *a.* The Facial nerves. These seem unaffected in this case; there is no dragging of the face to one side. In many similar examples this symptom occurs. It is to be noted, however, that it is often but of short duration; often in a few hours after the fit, the lateral deviation of the facial muscles disappears. Hence it is to be supposed that it is sympathetic and dependent on a radiated influence. It is usually on the same side as the extravasation; sometimes, however, on the opposite—a circumstance not yet elucidated by physiology. *b.* The Spinal nerves. The paralysis of the left arm and leg has already been noticed. It is worthy of remark that in most cases the leg regains its power of motion more quickly than the arm, and that the function of the nerves of sensation is less affected than that of the motor nerves. Dr. Neisser thinks this fact proves that the central axis of the sensitive nerves is not the same as that of the motor. He is of opinion that they end in the medulla oblongata, and that the disturbance of their function is sympathetic and dependent on a radiated influence. But even the motor nerves may be unequally influenced. A young man was placed under Schönlein's care, in the clinical hospital at Zurich, with incomplete hemiplegia of the left side, and lateral deviation of the facial muscles. The attack was acute; he had headache, &c., and it was pronounced to be a case of cerebral hemorrhage with congestion. A few days after, Professor Arnold was examining the affected muscles more minutely, and found that while the brachialis anticus was paralysed, the biceps could be freely moved. Now the two muscles are supplied with branches from the same nerve, and, therefore, Professor Arnold urged that it was a peripheral, and not a central or cerebral affection.* All the symptoms of the case were, however, opposed to this view, as well as the known fact that individual nerves, and they only, may be affected in diseases of the brain. The respiratory nerves are derived from the medulla oblongata, and their origin is, therefore, far distant from the seat of extravasation. We, however, find them affected in the case under examination; and the probable conclusion is, that they suffer in the same way as the hemispheres, namely, by irradiation. It is worthy of special notice that the respiratory muscles in this case are hemiplegic; for the left side of the thorax is motionless. *c.* The Sympathetic or Organic nerves participate in the sensorial disturbance; the bowels are constipated; the urinary bladder becomes distended; and the activity of the cardiac movements is diminished. Whenever the brain suffers from compression, the pulse becomes slower; the more indeed the former is compressed, the more the latter is retarded, becoming at the same time larger and fuller. This is the pulsus cephalicus of the ancients. To the experienced practitioner the condition of the pulse is of great importance in the diagnosis. It gives often the first warning of commencing cerebral disease. In those cases in which the hydropical effusion takes place slowly and insidiously, without any symptoms referrible to the head, the retardation of the pulse is the first symptom that indicates the impending danger.

Etiology of the affection. What is the cause of the apoplexy in this case? It cannot be ossification of the cerebral arteries, or softening of the brain, or the apoplectic predisposition. These belong to advancing years. Young individuals may have apoplexy from two causes; namely, hypertrophy of the left ventricle, and ectasis or dilatation of the cerebral arteries.

Ectasis vasorum cerebri is a form of disease of which little information has hitherto been gathered by systematic writers. Dr. Neisser has seen it in two young men, aged from 20 to 30, and in a female with amenorrhea, aged 19—all characterised by a plethoric habit, short neck, thick head, stunted growth, &c. The disease most frequently attacks the basilar artery and its branches. These are simply dilated equally along their whole length, and not in aneurismal pouches. The blood moves, consequently, more slowly along them; in fact, the disease is not easily distinguished in its early stage from active congestion dependent on other causes. The patient is readily heated, complains of overflow of blood, headache, sensual disorder. These symptoms recur continually, in spite of energetic and methodical treatment; and at length become inveterate, and excite anxiety. The patient complains at last of vertigo, and a peculiar heaviness and dulness of the feet. He has neither pain nor formication, but walks with a tottering, uncertain step, staggering first to one side, then to the other, as if intoxicated. He is often so affected with vertigo that he falls, particularly towards evening, and in the dark. All the symptoms are aggravated at night, and remit in the morning. There are partial sweats on the head and face, and in some cases amblyopic symptoms. Dr. Neisser has found an energetic antiphlogistic and revulsive method perseveringly carried out, and under favorable circumstances, to be entirely inefficacious. Death is generally sudden. There is no reason to presume the existence of either hypertrophy of the left ventricle, or ectasis of the basilar artery, in the case under consideration. The probability is, that the extravasation is connected with the rheumatism of the nuchal muscles, and that there has been a metastasis to the brain or membranes.

Prognosis of the case. The impression on first looking at the patient is unfavorable. The intensity and duration of the stage of unconsciousness is, perhaps, the best prognostic sign. If an apoplectic patient lies utterly insensible to all stimuli, breathing stertorously, with open mouth and turgid cheeks, passing stool and urine unconsciously, &c., death is inevitable. On the other hand, a transitory loss of consciousness, with slight paralysis, by no means augurs an unfavorable termination. It is in the mixed cases that the greatest uncertainty arises. Here the patient answers, although unintelligibly, when spoken to. Neither is he worse than he was twenty-four hours ago. So far the prognosis is favorable; but the inflammatory action is a serious complication, and renders the termination doubtful.

Treatment. Venesection to 12 ounces; ice to the head; internally, nitre with sulphate of potass. The bleeding to be repeated, if the symptoms are aggravated.

Friday, 17th. Let us accompany Dr. Neisser on his visit to his patient, and see what has been the result of the treatment. Bleeding was performed forthwith; the blood drawn had a firm clot, with two teaspoonfuls of serum. Immediately after the bleeding the patient opened his eyes; the pupils acted, and the paralysis of the left arm disappeared. A paralysis to disappear in two days! Was there really, then, extravasation of blood, or was the diagnosis of yesterday wrong, and there was only extreme cerebral congestion? Was it an apoplexia intra vasa, a paraplexia, a coup de sang? No; the paralysis is opposed to this opinion. In all forms of paraplexy, whether in plethoric young persons, or occurring as apoplectic molimina, there is very seldom, *if ever*, hemiplegia or complete paralysis. But the readiness with which the paralytic affection has disappeared points out another locality for the extravasation than that fixed on yesterday. Cases of sanguineous apoplexy are dangerous in proportion as the locality of the hemorrhage is distant from the surface of the brain. Hemorrhage at the base of the brain is the most dangerous and most fatal; hemorrhage on the surface of the convolutions is the least dangerous and least fatal. The latter occurs the most frequently in children, and constitutes the apoplexia neonatorum. The hemorrhage is usually consequent on exudation through the capillaries, or, according to Cruveilhier, from the smaller venous trunks—at least in children. In these examples, the apoplectic symptoms are much less dreaded and more gradual, than in other forms. The symptoms of the case in hand are of this character: the paralysis has been easily removed, the loss of consciousness is incomplete; the extravasation is not into the right thalamus and corpus striatum, but on the surface of the left hemisphere.

After a clyster, the patient had three motions; retching and vomiting took place whenever he drank liquids; he was restless, and had no sleep during the night. To-day he tosses his head about, sometimes his hands, sometimes his legs; he answers questions hastily and imperfectly, in a sort of half-delirious manner. The face is red, the eyes injected, the head and cheeks hot, the pupils contracted. It is found, however, that the nuchal muscles are no longer tense, and the head is moved about much more freely, and without pain; but there is considerable pain on pressure in the region of the third, fourth, and fifth cervical vertebræ. The breathing is quite otherwise than yesterday; it is frequent, deep, regular; the vomiting continues; the urine is scanty, and strongly saturated; the skin is hot and dry; the pulse is 92, large, and strong, but its rhythm is irregular; a few beats go quicker than the preceding, and then the rhythm becomes slower again—a significant symptom; a symptom to be carefully noted in all cerebral affections. The symptoms are now altogether changed; the diagnosis is difficult, but the grounds must now be stated for pronouncing it a case of acute arachnitis at the base of the brain, with commencing exudation of pus, and complicated with meningitis spinalis in the same (or purulent) stage.

What are the reasons for this diagnosis? The leading symptoms indicate to-day a state of cerebral irritation; we need not repeat them, but they were masked yesterday by the pressure on the brain. There was no restlessness, no active delirium, no shouting; the brain was in a state of torpor. Venesection removed the cause of this, and the true characters of the disease appeared. Now that this state of irritation is inflammatory is manifest from the febrile symptoms, and that it is not encephalitis is probable, because that disease is rare; and, further, the rheumatic affection of the nuchal muscles indicates the serous membrane as the locality of the inflammation. It is to be noted, too, that a rheumatic affection of the nuchal muscles is a prominent circumstance in the natural history of arachnitis.

The nature of the inflammation is such that it must have continued three days; the period, then, for serous exudation is past; it is the period of puriform exudation. Systematic writers generally describe the two stages as being distinctly marked, the second being the stage of compression; but this, Dr. Neisser declares, is quite wrong. There is always a transition-stage which complicates the diagnosis much, because it complicates the symptoms. The symptoms of compression are at first mingled with those of irritation, and it is only near the fatal termination of the case that the latter disappear. In the case before us the sensibility of the pupils, the active delirium, the expression of pain, and the vomiting, are all marks of irritation. Vomiting, like the peculiar pulse, is a specially important symptom in arachnitis. It is usually preceded in children by a tendency to yawn and by nausea; when fully established it is easily excited by the ingestion of medicine or food. The matter vomited is generally a watery mucus, occasionally mixed with bile, and it is rejected without much effort. Andral limits the symptom to the first, or purely inflammatory, stage of arachnitis; Dr. Neisser has found it frequently present in the second stage. He, therefore, concludes that the vomiting of yesterday indicated that the arachnitis had already attained the second stage, and that morbid products were formed. The febrile symptoms then observed were dependent upon the active inflammation going on; the compression of the brain masked the general symptoms of arachnoid inflammation.

The prognosis is now most unfavorable; there is no hope for the patient except in active treatment; arachnitis, if not checked, invariably conducts to death. In proposing a repetition of the bleeding, Dr. Neisser makes several practical remarks on that operation, for which we regret we cannot find room. Schönlein thinks that if the product-stage has arrived, all remedial means only hasten on the fatal termination. Dr. Neisser thinks, however, that cures of arachnitis in the second stage are sufficiently numerous, and that Schönlein is wrong in his therapeutics. Andral, Abercrombie, Portal, &c., forbid venesection, but recommend leeches. Dr. Neisser thinks that no rule can be laid down, but that leeches only can be of but little use in so intense and dangerous a form of inflammation as acute arachnitis. It is certain that bleeding in the latter stages may hasten on dissolution; the pulse becomes suddenly weak and fluttering, and the patient sinks rapidly. A careful diagnosis will, however, indicate the cases in which general bleeding is inadmissible. In the present example, Dr. Neisser prescribed bleeding to eight ounces, twenty leeches to the neck, ice to the head, calomel and jalap to act on the bowels.

Saturday evening, the 18th. The patient's relatives have stated what happened to him. On Tuesday he complained of hemiplegia, and on Wednesday he fell into the unconscious condition in which he was brought to the hospital. This circumstance confirms the diagnosis that the form and site of the hemorrhage was as last indicated; namely, capillary exudation on the surface of the left ventricle. The palsy preceded the loss of sensibility, and came on gradually—an important distinction. The patient did not, as was expected, experience any evil results from the venesection. The pulse was less irregular; the vomiting had ceased. In other respects the symptoms varied little. There was excessive pain in the neck, but more composure of manner; sleeplessness during the night, and active delirium. We shall not follow Dr. Neisser through his estimate of the symptoms; but the result is a different diagnosis to that on the preceding. Now it is arachnitis of the cervical cord extending to the medulla oblongata and base of the brain; in the first or inflammatory stage, the prognosis is more hopeful, but doubtful, on account of the persistent sleeplessness. These opinions seem principally based on the greater rationality and mental composure of the patient, a state of things incompatible with the more advanced stage of the disease. The increased cardiac action is manifest in the exquisitely inflammatory pulse, and indicate the necessity of a repetition of the bleeding, which is ordered to the extent of one pound, but without leeches. We give the result verbatim:

> "Sunday, 19th. We have a foreboding of evil as we approach the bed; there is an ambiguous character of repose and passiveness, an inexpressive eye. The pulse hurries on at the rate of 130, small, and weak. It is all over; the patient is dying. It is the last stage, the stage of exhaustion, the herald and forerunner of inevitable death. . . . . . . After the bleeding there was a gradual abatement of the excitement. The pulse lost its tension, but remained strong and good, and the whole appearance and condition of the patient was satisfactory. An evening exacerbation was to be expected under the existing circumstances, and an increase in the symptoms. It came, was rather severe; so that bleeding to six ounces was again resorted to. A satisfactory remission followed, and an improved condition. The patient also fell into a quiet sleep, which continued for two hours. Towards morning he again became restless, but weakly, and without energy. He muttered deliriously, the pulse sank, became frequent, profuse diarrhœa set in, and the stools were passed involuntarily.
>
> "Such is the history of the circumstances that now appear before us. He lies in a constrained position, apathetic, muttering to himself, whether addressed or not, unintelligibly, inarticulately; no trace of consciousness; the eyes dull and staring; the temperature neither increased nor diminished; the pulse as described."

Such is the closing scene of the pathological drama which Dr. Neisser has written for the instruction of his readers. To them, at least, the catastrophe is in the post-mortem. It is as follows:

The dura mater was injected, the inner membrane dark red; much congested; could only be raised up in large patches, and with difficulty; and a quantity of reddish serum was collected beneath the arachnoid. There was an extravasation over nearly the whole of the convolutions of the left hemisphere, consisting of a thin layer of blood, partly coagulated and partly fluid, and dark. Both the pia mater and the compressed convolutions were at this point much congested, but no arterial or venous lesion was perceptible. The cortical substance was very vascular, and the medullary showed numerous red points. The parietes of the lateral ventricles, the thalami, &c., were all healthy; the ventricles contained some bloody serum. At the base of the brain, and particularly about the medulla oblongata, and the adjoining portion of the cerebellum, there was a thick, dirty yellow fluid, a purulent exudation between the arachnoid membrane and pia mater, the latter being much reddened and infiltrated with a watery fluid. In the dorsal region of the spinal cord there was a well-marked reddening of the membranes, which contained between them a collection of a yellow, thick, putriform fluid. The adjoining portions of the cord were affected, but the pia mater was much less reddened and infiltrated in the cervical region than in the dorsal.

The Epikrisis. We have now before us a tolerably complete statement of all particulars connected with this instructive case. It remains to institute an epikrisis. We must review the symptoms and see where the observer was right, where wrong. In the first place, the opinion as to the existence of a cerebral hemorrhage, and the judgment founded on the gradual accession of the paralysis and loss of consciousness, or the less degree of their intensity, and on their ready removal, as to the seat of the hemorrhage, are both confirmed by the inspection of the corpse. It is also apparent that the hemorrhage was not from an opening in a vessel (diabrosis), but was an exudation from the capillaries (diapedesis), consequent probably on the febrile excitement and inflammatory congestion. This mode of hemorrhage is also indicated by the *gradual* accession of the phenomena; if consequent on the rupture of a blood-vessel, the fit is more sudden and instantaneous. The paralysis was on the same side as the hemorrhage; this is an interesting circumstance, and is certainly an exception to the general rule. We do not yet know sufficiently the relations of the various parts of the brain to explain the differences. It is at least worthy of notice, that when there is decussating hemiplegia from hemorrhage into the thalamus or corpus striatum, it happens that the facial nerves are affected as often on the same side as on the opposite. Now the facial nerves take their origin above the decussation in the pyramids; why then are the facial muscles not subject to the same rule of paralysis in hemiplegia as those of the trunk?

The patient was restored to consciousness immediately after the bleeding; it could not have effected a removal of the exuded blood, because no bleeding could have effected that. It must then have been by relieving the congestion. Various considerations lead certainly to the opinion that pressure alone cannot always or indeed usually induce the unconscious state. It is more probably the "shock." We know that in cases of injury to the head, the brain becomes accustomed to considerable pressure without any aberration of function. So also in chronic arachnitis (hydrocephalus) and in diseases of the brain characterised by hypertrophy.

The condition of the pulse in different stages of the cases is worthy notice: on the first day it was 104, inflammation predominant; on the second day 94, and arhythmical—a mixed stage of inflammatory action and purulent deposit on the brain; on the third day 105, tense and regular—pure, intense inflammation; on the fourth and last day 130, and weak—the brain exhausted.

There was dorsal meningitis, as intense as the cerebral, yet the most prominent symptom did not exist; there was not the slightest affection of the spinal nerves. Probably the attack was inevitably fatal. We believe, however, that the treatment was bad, and that it hastened the mortal event. The bleeding was carried much too far. We are of opinion that if the learned doctor had kept his reasoning to himself, and left Nature to her own devices, there would have been a better chance for his patient. He will know better ere he is threescore. But we must pass on to the second case, of which we must make shorter work than the last.

> "February 15th, 1839.—We come to the bedside of a young man who is unconscious, with his eyes closed, speechless, motionless, his head and neck spasmodically drawn to the left side. From time to time there is a spasmodic agitation of the hands and fingers. Respiration quick; pulse sunken, 144. *Peracute arachnitis at the base of the brain.* The inflammation is remarkably intense, and has attained already to considerable purulent formation. The disease has done its worst; it has exhausted the vital powers of the organism; the assistance of art is too late. It is the beginning of the end.
>
> "The patient, a young man aged 19, of delicate constitution and weak muscularity, was brought last evening to the Charité Hospital, delirious and unconscious. We learn that already for several days he has been in a delirious unconscious state, and has had a rose eruption on the feet. A physician was called to him yesterday, who ordered venesection and leeches to the forehead, and sent him here. After his arrival last night, he was attacked with opisthotonos for two hours, and was delirous the whole night. He is girt with a broad padded belt, to which soft leather rings are attached to receive the hand; a circumstance that convinces us he must have been raving violently, as these means of restraint are only used here as the last extremity, and when danger is apprehended.
>
> "Let the reader imagine a young man stretched on the back, his head turned to the left side, the eyes closed, the countenance placid, as if he were asleep. Only a loud frequent respiration and convulsive movements of the hands and fingers break at short intervals the singular stillness which impresses the mind. The extraordinary appearance of such a patient rivets the eyes, and shocks the senses as well as the feelings. The impression made by the morbid phenomena is so powerful, because the uncommon quietude, the death-like insensibility of the patient, together with the unchanging unnatural position of the head, contrast frightfully with the powerful convulsions and the hasty elevation of the hands; the patient appears inwardly fettered by some morbid power, and the soul of the spectator is filled with irrepressible sensations. . . . . He gives no answers to our questions, he takes no notice of us. . . . . The face exhibits no capillary congestion, nor any thing particular; it is neither remarkably hot, nor red, nor pale; the tone of the facial muscles is normal; neither the lips nor teeth are spasmodically compressed; there is no distortion of the mouth, no spasm of the eyelid, no lateral deviation of the face. We open the eyes—the patient is insensible to light, but the pupils are normal, and contract. The nuchal muscles are not swollen; if, however, we attempt to straighten the neck or the head, there is an expression of pain on the countenance, and the head is restored to its unnatural position. The respiration is loud and feverishly quick, but rhythmical, full, and deep; no vomiting; abdomen soft and natural; region of the bladder free. The extremities extended, but not paralysed. . . . . We can say nothing of the secretions (except that there is a perspirable skin), for the patient passes urine and fluid stools involuntarily." (p. 109.)

We have not space to follow our author through his diagnostic analysis of the phenomena in this case, his prognosis, his etiology, or his physiological estimate of the symptoms, in which he discusses several points in cerebral physiology, but go on to

The treatment of arachnitis by cold affusion and the actual cautery. Of course, the nature, intensity, and stage of the affection in this case show that it is hopeless, but Dr. Neisser takes occasion to discuss certain means most likely to be successful in arachnitis. These are two: the actual cautery and cold shower-bath.

The actual cautery has the appearance of being a most painful and severe remedy. But it is in fact quite the contrary. It is surprising to observe, when applied without previously acquainting the patient with the intent, how little pain it gives. Dr. Neisser observes, that he has seen even children endure it without a cry, or appearing conscious of its application. We have ourselves been astonished to hear no expressions of pain when the living flesh has been hissing and frizzling beneath the glowing iron. Dr. Neisser states, that he has known men in perfect possession of their natural sensibility describe cauterizations several inches long as causing simply a sensation as of warm water being applied. Its remedial power by no means consists in a sudden shock to the system, for this it does not effect. It is an active derivant and counter-irritant. Dr. Neisser has found the actual cautery to act like a charm in checking deep-seated inflammation and exciting absorption, or rather in affording an opportunity to the so-called vis medicatrix to come into play. In diseases of the bones and articulations, its utility is great. Dr. Neisser founds an opinion as to its *modus operandi* on the well-known tendency to metastasis, exhibited by inflammation of serous membranes. The artificial irritation determines a metastasis of the original inflammation to the part irritated. It is necessary, however, that there be a thickness to muscle, or of cellular tissue between the cauterized and the inflamed part; otherwise it will only aggravate the existing inflammation. Its application to the head, or to the elbow, knee, feet, and fingers is inadmissible. Nor if the fever be violent should it be used, as it will, under such circumstances, be more injurious as a direct irritant than useful as a counter-irritant. It ought not, therefore, to be used in cerebral affections accompanied with active delirium and great irritability.

The effects of the cold douche, or shower-bath, are remarkable. Applied to a patient who is unconscious and insensible (as in the example under consideration), its first effect is a powerfully shuddering impression. The patient appears terrified, is aroused for a moment from stupor, sees, hears, and sometimes speaks; he complains, is restless, &c. Children especially exhibit great anxiety, they shiver, their teeth chatter, they cry aloud, weep, &c. If repeated, the same phenomena again appear, and the surface becomes cold, the skin contracted (*cutis anserina*), and the blood seems concentrated in the heart and lungs. The respiration is hurried, the pulse sinks. If the patient be rubbed dry and carried to a warm bed, in a moment or two all these urgent symptoms gradually disappear; the pulse regains its tone, the skin becomes warm, but instead of its former burning heat, it is now moist and agreeable. The kidneys secrete urine profusely, and the patient feels so comfortable and so much relieved, that he sinks into a gentle slumber, and will often request a repetition of the douche.

The cold douche is, however, a most potent depressing remedy, and should be used with great caution. Dr. Neisser says, that if continued long enough, it will induce so great prostration in the strongest man, that reaction will not easily be brought about. The frequency of its repetition, and the quantity and temperature of the water are to be decided by circumstances. The greater the heat of the patient, the cooler may be the douche. For an adult, from three to eight pailfuls of water may be used; and for a child, the same number of pitcherfuls, continued from two to fifteen minutes, according to the strength of the patient and the effect produced. A child should be placed in a tub half filled with warm water. The skin, if it perspires, should be carefully wiped dry, and the chest and neck wrapped up in a napkin to prevent the cold water touching them. This precaution should never be omitted, but it is doubly necessary when the disease is complicated with a thoracic affection or tendency to tubercular deposit. A little water should be first sprinkled on the child's face, and then poured in a broad stream on its head; an interval of a few seconds should elapse between each affusion. When the operation is completed, the child should be quickly rubbed dry, wrapped in a warm blanket or flannel, and put to bed. Dr. Neisser thinks the practitioner should himself perform the operation, or, at least, personally superintend it, and see the patient shortly afterwards.

Moderate cold affusion is a useful remedy in many cerebral affections. In passive hyperæmia or venous oppletion, such as is met with in drunkards, in delirium tremens, in narcotic poisoning, in congestion consequent on exanthematic fevers, and in low fevers (febris nervosa stupida). It is also a very valuable remedy in irritable states of the brain with active congestion, and in the premonitory stage of apoplexy. It is strongly contraindicated, however, in all cases where the debility is great; it is, therefore, inadmissible in the last stage of arachnitis. The end of the first, and the transition stage to the second, are the periods when it is most likely to be useful in that disease. In mania, and in all cases in which erethism of the brain is intense, the slightest degree of cold affusion is strongly antiphlogistic, and may be used and continued, until that state of prostration is attained to which we have before alluded. Care, however, must be taken with this, as with all other lowering means, that it be not carried too far.

After this long digression (and we are bound to say that Dr. Neisser makes a much longer, for he discusses at length the physiological action of cold affusion) we will return to the case in hand. It is apparently hopeless; but the zealous practitioner is true to his life-saving *métier*—"while there is life there is hope," is the maxim most deeply impressed on his mind. To gain time in acute affections is a leading indication; to push on the clogging weary wheels of life is his main object, until mayhap the cycle of disease is completed, and the struggle ends in victory.

The treatment Dr. Neisser adopted in this case was cold affusion, and the actual cautery to the nucha; ethereal oil of mustard to the scalp; poultices sprinkled with camphorated spirit to the whole surface of the leg. The patient died on the 16th.

Post-mortem examination. The dura mater was sound; the vessels filled with fluid blood. On the convex surface of the brain the membranes had a red appearance, in consequence of injection of the vessel of the pia mater; a small quantity of reddish serum was effused between the latter and the arachnoid. The substance of the brain was firm; the ventricles containing serum, but not distended beneath the arachnoid membrane at the base of the brain; there was a layer of a whitish green puriform fluid, which covered the under surface of the anterior lobes of the brain, the fissura Sylvii, the olfactory nerves, together with the pons, medulla oblongata, and the adjoining surface of the cerebellum. The subjacent brain was reddened, but not softened; the pia mater was not adherent to it. The spinal arachnoid membrane was filled with a flocky serum; the posterior surface of the upper cervical portion of the cord was covered with a purulent exudation, like that found at the base of the brain, and (like it) contained between the pia mater and arachnoid, the former membrane being tumefied and infiltrated.

The leg was inflamed and infiltrated with pus and dark red serum.

Complications of arachnitis. The next case is interesting, as an example of arachnitis complicated with certain morbid changes in the brain. The patient was a confirmed drunkard, had already suffered from delirium tremens, and was sent to the hospital by the attending physician as labouring under that disease. The characteristics of the delirium, peculiar to the latter disease, were wanting, while the other symptoms pointed at an inflammatory affection, but so doubtfully, that it was manifest there was some complication necessary—some complication of the disease which ought to be unravelled. There are several complications: the first is arachnitis with inflammation of the cortical substance of the brain; this occurs in about one half the cases of arachnitis; it does not, however, alter the series of symptoms. Another complication of arachnitis is well-marked congestion of the brain. Congestion of the cortical substance is probably a complication of nearly every case of arachnitis, on account of the intimate and direct vascular connexion between the pia mater and the brain; but this again makes no change in the symptoms. It is quite otherwise with the much rarer complication of arachnitis, namely, congestion of the medullary substance. In these cases the symptoms of excitement are masked by the congestion until after bleeding, when active delirium sets in, and the patient is apparently worse, although the change is really due to the removal of the pressure.

There are examples of congestion of the capillaries complicating arachnitis; but there is also a venous congestion, a hyperæmia of the cerebral veins. This complication is not often alluded to by systematic writers, but it is an important one; it is usually seen as the sequel of all habitual congestive states of the brain, but is never so constant as after the abuse of alcoholic drinks. In death from delirium tremens, the venous sinuses are not only turgid with blood, but the veins generally even to the small branches in the pia mater are congested and turgid, often giving rise to a thin lymphatic exudation, which is to be distinguished from inflammatory exudation, and not confounded with it, as has been done by some writers. A similar exudation takes place to a more or less extent in all parts of the brain as well as into the ventricles. The symptoms in the case under consideration tally with those which might be expected to occur in such a pathological state, but with this difference, that an actively inflammatory condition has aggravated the state of congestion, while the symptoms of the former have been masked by the latter. The diagnosis then is purulent arachnitis at the base of the brain, with serous effusion into the ventricles, caused by hyperæmia of the brain.

The prognosis was of course in such a case highly unfavorable. The treatment was as follows: 20 leeches were ordered to be applied to the head, and four grains of calomel and ten of jalap every three or four hours; on the next day 15 more leeches, and the cold affusion carefully applied; on the third day of treatment the symptoms became more threatening, and the patient gradually sunk until the evening of the seventh, when he died comatose. We will now proceed to detail the post-mortem examination, and see if the diagnosis was correct.

Post-mortem appearances in the brain of a drunkard dead of arachnitis. A quantity of clear serum was found effused between the two serous membranes; these were quite distended with serum, the arachnoid having a bladder-like appearance, besides being full of blood, in consequence of the highly congested and distended state of the veins over the whole convex surface of the hemispheres. Between the pia mater and arachnoid, besides a clear serum, there were found delicate fibrinous coagula in the form of narrow and broad threads adherent to the veins. Some of the venous trunks had a true varicose appearance. The cortical substance of the hemispheres was dark red, but otherwise healthy; so also the first slices through the medullary substance. On going deeper, however, the corpus callosum, with the adjacent septum pellucidum, and fornix were found much softened, and fell under the touch into a pulpy mass of a dull tinge, neither yellow nor red. The lateral ventricles were enormously distended with serum. The corpora striata and thalami were normal. At the base of the brain the arachnoid membrane had a dull pearly appearance, and beneath it a quantity of cloudy flocculent serum was collected; upon the medulla oblongata and cerebellum there was a quantity of milky puriform fluid. The spinal arachnoid membrane in the dorsal and lumbar regions was distended with a flocculent cloudy serum. At two points of the spinal cord, in the cervical and lumbar regions, the pia mater was injected, and the corresponding portion of the cord was softened, so that the knife passed through it as if it were gelatinous. The liver had undergone the fatty degeneration; the mucous coat of the stomach was softened; the small intestines exhibited slight intussusception, which were however easily unravelled by the finger.

It is to be remarked in this case, that there was softening of the brain and spinal cord; a condition not indicated by the symptoms. Dr. Neisser observes, that in this respect it is analogous to one half the cases of hydrocephalus, in which proportion there is a similar softening of the central parts of the brain, without any corresponding symptoms during life. Dr. Neisser supposes that this morbid change occurred a day or two before death, during the period of collapse.

The next case is one of a different character to any of the preceding. It is that of a journeyman weaver, aged 46, admitted into the hospital on July 31st, 1839. Dr. Neisser describes his appearance thus:

> "His complexion and appearance is that of health; his expression intelligent and vivacious. Left to himself, there is nothing in his position, demeanour, or expression which would attract the observer's attention. To our inquiry as to what he suffers, and how long he has been ill, he answers very quickly in a loud tone, just as if he had been accustomed to converse with a deaf person; but he hears perfectly well, indeed listens attentively, and what he says is perfectly in-

telligent and connected. He states, that about fourteen days ago, he was chilled while bathing, experienced rigors and heats, and had suffered from headache, and particularly from sleeplessness and restless nights; in other respects he is healthy. The hands are hotter than natural, dry and rough. This, in conjunction with the state of the pulse (96-100), shows a certain amount of general febrile action. Tongue, thorax, and abdomen offer no symptoms." (p. 305.)

Dr. Neisser remarks on this case, that it is a remarkable example of disease in this respect, that only two or three days remain to elapse before the patient will be on the brink of a precipice, and yet he has almost all the appearances of health. The inexperienced practitioner would possibly consider it (recollecting the alleged cause) as a rheumatic fever, and would treat it with diaphoretics. He would hasten the fatal termination, and would be praised for his unremitting attention.

The peculiarities that distinguish this case from the preceding are the exalted sensibility of the sensorium, the quickness of the sensual perceptions, and the hastiness of manner. These, together with the other symptoms, point out a cerebral affection differing considerably from those previously narrated. On a more minute examination it is seen that there is some little injection and swelling of the eyelids; this symptom is significant, it indicates congestion of the head of some duration. There is also dilatation of the pupils, which continues even when the patient is opposite to a strong light. When the patient is raised and made to sit up, he complains of vertigo, and experiences heaviness and confusion of the head, so that he cannot keep himself up. There is, however, no tendency to vomiting. There is no sympton of paralysis in any of the limbs; the breathing is deep and regular.

The symptoms manifestly point to an inflammatory affection of the encephalon. The inflammation is not, however, peracute, having already existed 14 days; it will not terminate in the production of puriform exudation, for if the inflammation were of this character, death would already have ensued. It is therefore a subacute form, accompanied by and ending in a serous exudation. The increased temperature, the frequent and regular pulse, the regular breathing, the general irritability, all indicate the first stage of inflammatory excitement to be predominant. There are other symptoms which show that effusion has commenced, and the stage of oppression entered upon. The headache is replaced by vertigo and confusion of the head, experienced especially when the patient raises the latter. The dilatation of the pupils is a noticeable symptom, although Dr. Neisser thinks that too much pathognomonic importance has been attached to it. In fact, we are not yet certain under what circumstances the pupil contracts or dilates. In cases of this kind the effusion goes on for some time before it is indicated by well-marked symptoms. The brain (already excited) resists the oppressive effect of the effused fluid longer than it would if in its normal condition, so that the symptoms are not in proportion to the effusion. There is, indeed, a sort of interval, a point of counterpoise between the two agencies, which may be detected by the equal balance between the two classes of symptoms.

Prognosis. If the disease be left to take its course, and the patient be abandoned to the healing powers of Nature, he dies. The vis medicatrix is here a fiction. The pulse is weak, yet the patient talks strongly, breathes freely, moves actively, has no expression of debility about him. There would appear to be a contradiction here; nevertheless, the pulse is to be taken as a true index that the stage of exhaustion is not far off. This circumstance renders the prognosis unfavorable, and the more unfavorable, because the means necessary for the removal of the disease are such as will still further reduce the powers of the patient.

Treatment. The disease being still in the inflammatory stage, no absorption of the effused serum can take place; on the contrary, the quantity is hourly increased. This inflammatory action cannot be removed without the loss of blood, and the question arises as to the mode; that is to say, whether by leeches or venesection. Now the weakness of the pulse forbids general bleeding. In no class of diseases is this remedy so dangerous as in meningeal inflammation. Dr. Neisser explains it physiologically after this manner. Every period of activity continued and prolonged, without the interruption of an antagonistic period of repose, must necessarily lead to exhaustion. Repose is necessary to the repair of the vital mechanism. In meningeal inflammations the patient (that is, his brain) is deprived of this necessary repose, as in the case under notice. For this and other reasons which will occur to the reader, bleeding from the arm is not advisable. The treatment, then, is ten leeches to the temples (five to each) toward evening, to anticipate the evening's exacerbations; ice-cold applications, but no counter-irritants, as these will only add to the existing irritation. Action on the bowels and kidneys should be induced; and for this purpose a tablespoonful of the following mixture is to be taken every two hours. We give the prescription verbatim: "℞. Magnes. sulphur, ℥ss.; S. in aq. comm. ℥v. Adde—Oxym. squill, ℥j.; succ. liquor, ʒiij., M." The result of this method we will also state in the author's own words:

> "Thursday, August 1st. In spite of the leeches and the ice-cold applications, there was febrile exacerbation, an increased heat of skin, and a pulse at 112, but no perceptible result of increased congestion. About ten o'clock the patient became very vivacious, sang a good deal, spoke much and loudly, then slept till one o'clock, when he awoke, and began to sing again. There have been four watery stools, but little urine passed.
>
> "To-day the pulse is 92. When addressed the patient answers in good spirits; 'he is perfectly well, ails nothing at all, has no pain, and will soon be well,' &c.; and while he is thus talking, the pulse becomes stronger, hard, more frequent; but so soon as he is quiet the pulse falls again. Nevertheless, he is not worse than yesterday, but somewhat better." (318.)

Dr. Neisser infers from the nocturnal exacerbation and the state of the pulse, that there is yet considerable cerebral action, and orders ten more leeches. The bowels having been well moved, he would now act on the kidneys by means of squills, as in the following formula: "℞. Acet. squill. ℥ij.; kali carb. q. s. ad perf. saturat. Cui adde Aq. font.; syr. alth. ana, ℥j."

This mixture, we presume, was taken as the previous one.

In the evening no exacerbation took place, the pulse fell to 50, with a good night and several hours of sleep. On the day following, the pulse at 60, the patient lay slumbering, and in breathing puffed his lips and cheeks; a serious symptom, as indicative of incipient paralysis. Cold affusion was now ordered instead of ice: a large blister to the nape, and an aperient. In the evening of this day vomiting took place twice, and the patient slept a couple of hours only.

On August 3d, the symptoms were more threatening; the patient puffed his cheeks still in breathing, the pulse only 52, the respiration cephalic; that dangerous form already noticed. The patient is less vivacious, but the intellect is little affected, and the disturbance being trifling in comparison with the other symptoms of compression, it indicates the effusion to be rather in the periphery than the base of the brain. The douche to be repeated, the blister kept open, the bowels and kidneys to be more stimulated. As the stomach is irritable, the squill must be omitted; calomel and jalap are, for this cause, inadmissible also; the following is a better dose: ℞. Tinct. colocynth, ℈ij; Aq. font. ℥iij; Syr. de spin. cervin. ℥j; a tablespoonful to be taken every two hours, according to circumstances.

From this period the patient began to recover. On a slight recurrence of the symptoms of compression, the following epispastic was sedulously applied to the head. ℞. Olei sinap. æther.; gtt. xxx; Spir. vini rect. ℥j. M. The urgent symptoms gradually subsided, and in eight days the patient was convalescent.

Hydrocephalus acutus is the next case. The patient was a lithographic printer, aged 37. His symptoms were headache and darting pains through the head; weariness, loss of appetite. The complexion is muddy, the expression thoughtful and anxious; when raised up he could scarcely support his head; was affected with nausea, vertigo, and little or no fever; pulse 72, irregular; skin dry, urine scanty and high-coloured. The patient reeled and tottered as he walked, so much so that he would have fallen unless supported: this is an important symptom, because indicative of pressure on the brain. The symptoms enumerated constitute the prodromi of acute hydrocephalus. The disease is frequent in adults, and not confined to children exclusively, as has often been stated. In the latter, in addition to the symptoms enumerated, there is often tenderness of the abdomen, which, in conjunction with the vomiting, sometimes leads to an erroneous diagnosis. Dr. Neisser thinks the first stage of hydrocephalus consists in general meningeal congestion: in the next stage, inflammation sets in, and the subsequent stages are as in arachnitis. It frequently happens, that the effusion takes place suddenly and with great rapidity.

Dr. Neisser began the treatment in this case with a general bleeding to sixteen ounces; purgation with compound infusion of senna; ice to the head, and a warm bath, temperature not higher than 26° Reaumur (90° Fahr.), the patient wearing an ice-cap while in it. After the bleeding, &c., the irregularity in the pulse disappeared, with the tendency to vomit. The bowels were freely moved, but the bath had no effect on the skin. Towards evening the patient complained of excessive pain in all his limbs, but striking particularly from the head through the neck and back to the feet, whenever he made the slightest attempt to sit up, or to move the head from one side to the other. There was considerable fever; the pulse 120, and active delirium, so that it was necessary to restrain the patient. In the morning the pulse fell to 84, the delirium and pain had ceased, and nothing remained but a sense of soreness in the nuchal muscles. There was no tenderness to the touch. Dr. Neisser attributes the apparent exacerbation of the symptoms to the removal of pressure from the brain by the bleeding and other measures, and thus allowing the results of the cerebral irritation (previously masked by the compression) to show themselves. The pain in the back and feet was dependent on irritation from congestion of the spinal meninges. Remedial means: cupping to seven or eight ounces (fifteen cupping glasses), then a bath, and if an exacerbation comes on at eight, a small general bleeding. Internally to take nitre and tartar emetic. The day following, the pain of the neck had gone, the pulse was 72, skin normal, mind composed. The previous night had been restless. The treatment was expectant: on the evening of the third day, symptoms of collapse began to show themselves; there was also a return of the pain in all the limbs, and in the neck and back; and that he might not increase it, he remained motionless. On attempting to raise him, he complained much, but there was no spinal tenderness. The pulse was 72, and little fever. Dr. Neisser now noted the case, as hydrocephalus complicated with arachnitis spinalis. It is worthy remark, that this apparent irregularity in the course and development of that disease, is in fact its normal course. Dr. Neisser now prescribed a bath with three pounds of carbonate of potass, and the following mixture: ℞. Liq. kali acet ʒj; Oxym. scill: ℥ij. Aq. font. ℥iij: a tablespoonful every two hours. The following ointment as a counter-irritant to the spine: ℞. Ung. mercur.—Ung. antim. tart. ana ʒss. M.

From this period the patient began to recover, and was dismissed convalescent on the twelfth day of treatment.

The next, or 6th case, is one of arachnitis with effusion, four weeks after a fall from a stool. The 7th case is one of arachnitis spinalis with serous effusion, inducing paraplegia. We regret that our extended analysis of the preceding histories of disease renders any notice of these impossible. We will therefore pass on to the epilegomena, and cull a few practical or other interesting remarks.

General notanda of arachnitis. The first stage of an acute arachnitis is characterised by developed cerebral function in consequence of irritation; when the inflammation is passing into suppuration or exudation of pus, the symptoms of irritation are at first mixed up with symptoms of pressure on the brain: they then gradually disappear, and at last the latter only remain.

A difference in the degree of inflammatory action determines the nature of the inflammatory product: the subacute is characterised by serous exudation, the peracute by purulent. The subacute form its more gradual and imperceptible in its course to a fatal termination; in the peracute the successive stages of the disease rapidly follow each other, and the symptoms of cerebral excitement and the general inflammatory reaction in the system is more strongly developed. The earlier stages of the subacute disease are often insidious, and considerable effusion takes place before there is any very manifest disturbance of cerebral function. Occasionally, indeed, when the inflammatory action is both subacute and circumscribed, symptoms of pressure on the brain are the first indicants of the disease. If, on the other hand, the inflammation be general, but of low intensity, it constitutes another distinct and well-marked variety of arachnitis, known as the premonitory stage of acute hydrocephalus.

The complications of arachnitis are—1. With venous hyperæmia of the brain. This is usually seen in drunkards, and leads to a mixed form, in which there is neither an exclusively serous nor puriform exudation, but a combination of the two. If, however, the inflammation thus complicated be peracute, that dangerous form of disease is developed, known as apoplexia serosa. A second complication of arachnitis is with hemorrhagic apoplexy, or, in other words, an extravasation of blood into the substance, or on the surface of the brain. In this case, the symptoms of irritation are repressed by the pressure on the brain, and are only developed when the cause of that pressure is removed. A third complication of arachnitis is congestion of the brain. In this the symptoms vary according to the degree of congestion, and the case is most intense; they are analogous to those of arachnitis complicated with serous apoplexy. These general views are applicable also to spinal arachnitis.

The physiological inferences deducible from these cases of arachnitis are various. Firstly, it is shown that the symptoms of pressure on the brain are not urgent in proportion to the amount of extravasated blood or effused fluid, but have a relation to the rapidity or suddenness with which it occurs. This is a general principle, applicable to all agents injuriously acting on the brain, as moral impressions, &c. Secondly, there is a greater loss of consciousness when the inflammation and its product are situate at the base of the brain than when on the convex or lateral surfaces. It is thus also with cerebral hemorrhages; hemorrhage at the base induces the most profound and incurable coma; hemorrhage into or near the ventricles induces a less profound and more curable coma; the coma from hemorrhage on the periphery is the lightest and the most manageable of all. This general fact is capable of a satisfactory solution, if we adopt the doctrines of *reflex cerebral action;* and, so far from being opposed to the general opinion, that the mental acts originate in the hemispheres, may be made to harmonize with it. The incident excitor action, which excites the changes in the hemispheres necessary to mental acts, is cut off in its course from the periphery to the centre by the pressure on the conducting fibrils as they pass up from the cord along the base of the brain. A pressure here, indeed, induces a state of the hemispheres analogous to that in sleep; the stimulus from without is stopped. Now, when there is pressure from effusion on the hemispheres, it is only a counter-pressure that is exercised on the base of the brain; and so soon as this is alleviated, the channel of incident excitor impressions is again opened, and pressure is now only exercised on the upper convolutions, a considerable portion of which we know may be injured, and even removed, without any impairment of consciousness whatever. Coma, then, results rather from interruption of function, consequent on an interruption of the flow of the natural stimulus to the brain from the periphery, than on a derangement of structure; it is a result of pressure on the conducting media, rather than on the acting fibres. The same views will of course explain those phenomena (as irregular action of the heart, of the respiratory mechanism, &c.) which are dependent on an obstruction of the cerebral stimulus, in whatever that may consist; while the pressure on the conducting structures at the base interrupts the course of the incident excitor stimulus to the brain, it cuts off the supply of reflex cerebral stimulus to parts below. There is some consideration due, however, to the fact, that inflammations at the base of the brain are usually the most intense, and the extravasations the greatest, and consequently that the pressure under these circumstances will be greatest. But this circumstance is opposed to the well-known tolerance of injury which the hemispheres exhibit, so that, after all, the inference must be, that the greater the effusion, the greater the pressure on the conducting strands at the base of the brain.

To give the results more in detail. If there be apoplexy at the base of the brain, there is profound and persistent coma, paralysis of all the limbs, paraplegia, and generally a fatal termination. If there be inflammation at the base of the brain, violent spasms, namely, opisthotonos and trismus, followed by deep coma. If there be structural disease at the base, sensorial disturbance, great tendency to vertigo and syncope, titubation, &c.

Paralysis of the extremities may disappear, even when softening of the brain or spinal cord has taken place. A sufficient number of accurately observed cases have now established this principle, which sets aside the more current opinion, that in all cases in which paralysis or spasm is transient, there is *no* structural change in the nervous centres. At the same time, it cannot be denied, that in cases of arachnitis, such lesions of motility are sympathetic, and depend on an irradiation of morbid influence. It is a practical principle that spasm generally indicates a state of irritation in the nervous centres, and paralysis the contrary.

A circumstance worthy consideration is, that while the bowels are torpid in diseases of the brain, the stomach is irritable. Vomiting, as has been seen in the cases detailed, is an usual symptom in arachnitis. It generally accompanies concussion of the brain, and those chronic diseases of that organ which induce pressure. The physiological explanation is, that the vagus is affected at its roots by irradiation from the diseased portion; but then comes the question, why are the gastric branches only affected? Why, in short, is there cerebral vomiting, but not a cerebral cough? There must be some as yet unknown anatomical arrangement of the twigs making up the vagus.

The termination of the sympathetic nerves in the brain is demonstrated by pathology. The heart, intestinal canal, and urinary organs, all participate in the disturbance of the system caused by changes in the central axis. The pulse becomes irregular, the intestines torpid, the sphincter ani and bladder paralysed. The connexion between the brain and sympathetic system leads to remarkable modifications in the symptoms of certain diseases. Thus, in abdominal typhus, when active delirium sets in, the diarrhea, tenderness of the abdomen, &c., subside. If, in the course of a phthisis pulmonalis, cerebral disease occurs, and the patient becomes furiously delirious,—a complication we have occasionally observed, the cough, expectoration, and dyspnea disappear. We have heard a phthisical patient under these circumstances vociferate loudly, talk incessantly, be free from cough and from all rational signs whatever of disease of the lungs, when a post-mortem examination, made within forty-eight hours, has revealed the most extensive tubercular disorganization, so extensive, indeed, that there did not appear six inches square of healthy lung in the whole chest. In the same way we have observed extensive pneumonia to run through its course altogether unobserved and unsuspected by the practitioner in ordinary, until the stethoscope has demonstrated to him the presence of great and irremediable disease within the thorax. Again, we have observed a peracute gastritis to coexist with arachnitis: the stomach has been found after death so injected as to resemble a piece of red velvet, and yet during life there was not one symptom observed that could be traced to a morbid condition of that viscus.

Some of these complications may accelerate the fatal termination in arachnitis. The influence radiated on the pulmonary branches of the vagus may be so great as just to arrest irritation of the incident excitor nerves, and to prevent fits of coughing, but may not paralyse the organic nerves, and stop secretion. Then the mucus accumulates in the bronchi, and the patient dies at last suffocated. This state of things may not exist during the waking condition, but may, and indeed often does, supervene with sleep. In these cases sleep is dangerous, and, if the patient be not roused from it from time to time, will terminate in death.

Dr. Neisser insists that encephalitis never occurs except as the result of mechanical injuries, or as a complication of arachnitis, when it is confined to the portion of brain adjoining the inflamed membrane. The same remark applies to myelitis. But, under the term arachnitis, he means to describe a disease which has its true and primary seat in the pia mater, and which implicates the arachnoid membrane consecutively. The latter he looks upon as pathologically the cellular tissue of the pia mater. These membranes conjoined form the substratum of all idiopathic inflammation within the encephalon, while inflammation of the dura mater and of the substance of the brain follows only on mechanical injuries. We state these opinions doubtfully, and rather with the intent that our readers may know what is current among German pathologists, than for their adoption and approval. If the new views could be entertained which describe the arachnoid as a membraneous plexus of organic nerves, the pathology of the serous inflammations we have discussed must have a complete and minute reviewal. Possibly, indeed, there may be a clue obtained in this way to those inappreciable changes in the brain which give rise to delirium and other sensorial phenomena.

We have now to state in conclusion, that, after a careful perusal and analysis of Dr. Neisser's volume, we find it to be a valuable contribution to the natural history of disease. We cannot, indeed, recommend a more efficient plan than that which he adopts for the elucidation of obscure points in, and the general advancement of, pathology. The estimate of the symptoms, and their relations from the results of experience and the data of physiological science must be peculiarly rich in results. There is one great defect in Dr. Neisser's method, and that is, no attention is given to the etiological relations of the diseases he describes. We find no chemical analysis of the urine, no investigation into the condition of the blood, no consideration of the causal antecedents. The whole treatment is unmitigated empiricism.

Art. IX.

On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart. By George Burrows, m. d., late Fellow of Caius College, Cambridge, Fellow of the Royal College of Physicians, London, Physician and Lecturer on the Principles and Practice of Medicine at St. Bartholomew's Hospital.—*London*, 1846. 8vo, pp. 220.

The sensations experienced by all persons on placing the head in a dependent position, or on otherwise favoring congestion of the cerebral vascular system, are so determinate, that it has been almost universally received as a fixed principle, that the amount of blood within the cranium may be increased. And yet it cannot be denied, than an opposite opinion has been broached and advocated, for it seems to be a general law in metaphysics, that every principle, whether true or false, shall have its antagonistic principle. In the present work, Dr. Burrows aims at a twofold object: he proposes to show, firstly, that this opposite opinion is erroneous; and, secondly, that changes in the circulation within the cranium have a much wider pathological scope than they are generally supposed to possess.

The doctrine that the quantity of blood circulating within the cranium is, under all circumstances, a fixed quantity, is an offset from the iatro-mathematical school of Borelli, and was promulgated by Dr. A. Monro at Edinburgh during the latter half of the last century. It was founded upon the incompressibility of the substance of the brain, and upon the spheroidal form of the skull. The idea might probably be traced from Monro to Boerhaave, or Pitcairn, or to some of their disciples. When, however, the last of the iatro-mathematical school had disappeared, we find Dr. Kellie taking up and defending this one of its doctrines which Monro had promulgated, and enlisting the late Dr. Abercrombie amongst *his* disciples. The name of Dr. Abercrombie was a tower of strength, and gave renewed vigor to the iatro-mathematical views. Dr. Clutterbuck adopted them in his article on Cerebral Apoplexy, in the 'Cyclopædia of Practical Medicine,' and even Dr. Watson, in his very recently delivered Lectures, maintained the invariableness of the quantity of blood in the brain, adducing the argument which Dr. Pitcairn, or even Borelli, would probably have advanced. "This depends," remarks Dr. Watson, "upon the mechanical structure of the cranium, and is capable of explanation upon the known principle of hydraulics." It is but justice to Dr. Watson, however, to observe, that in the second edition of his Lectures, he acknowledges a change of opinion on this point in consequence of Dr. Burrows's demonstration to the contrary.

The above history of this heresy, sketchy and slight as it is, shows that erroneous ideas reappear like weeds, and are as apt to perpetuate themselves in the soil of science. Dr. Burrows was impressed with the idea, that the opinions promulgated by Dr. Kellie and his successors were erroneous, and that they obscured the pathology of a most important organ; he therefore repeated the experiments of Dr. Kellie, and in the present work we have the results of his inquiries. These results, as well as the pathological views of Dr. Burrows, we now propose to place before our readers.

Dr. Kellie inferred from his experiments—1, that a state of bloodlessness is not discovered in the brains of animals which have died by hemorrhage, but, on the contrary, very commonly a state of venous cerebral congestion; 2, that the quantity of blood in the cerebral vessels is not affected by gravitation or posture of the head; 3, that congestion of the cerebral vessels is not found in those instances where it might be most expected, as in persons who die by hanging, strangulation, suffocation, &c.; 4, that, if there be repletion or depletion of one set of vessels (arteries or veins) in the cranium, there will be an opposite condition of the other set of vessels.

In examining these experiments analytically, Dr. Burrows clearly shows, that they do not bear out Dr. Kellie in the inferences he deduced from them, and that, in fact, Dr. Kellie did himself at first draw the inference that the brain of animals might be depleted by bleeding, and their vessels drained of a very sensible proportion of the blood contained in them. It was in a subsequent communication to the Medico-Chirurgical Society of Edinburgh that he contradicted himself. It appears that his experiments stand alone, and have not been repeated: Dr. Burrows's researches are therefore the more interesting. We subjoin the first *in extenso:*

> "On the 11th of January, 1843, I killed two well-grown rabbits. The one (A, plate 1), by opening the jugular vein and carotid artery on one side of the throat; the other (B, plate 2) was strangled. Each animal died violently convulsed. A ligature was drawn tightly round the throat of the rabbit (A) immediately it expired, to prevent any further escape of blood from the vessels of the head. The rabbits were allowed to remain twenty-four hours on a table, resting on their sides.
>
> "While the blood was flowing from the rabbit (A), the conjunctiva was observed to become pallid and the eyeballs to shrink within the sockets. Upon the examination of the head of this rabbit, the integuments and muscles appeared blanched and exsanguined. Upon removing the upper portions of the cranium, the membranes of the brain were found pallid, and scarcely the trace of a blood-vessel was to be detected on the surface of the brain. The longitudinal and lateral sinuses were nearly empty of blood, and their course was not denoted by any colour of blood. Upon making sections of the brain, the interior appeared equally exsanguined.
>
> "Soon after the cord was drawn tight round the throat of the rabbit (B), the conjunctival vessels became congested, the eyeballs turgid, prominent and even projecting beyond the margin of their sockets. The integuments and muscles of the head were found full of blood. Upon opening the cranium, the superficial vessels of the membranes, as well as the sinuses, were full of dark liquid blood. The whole substance of this brain and its membranes appeared of a dark reddish hue, as if stained by extravasated blood. The contrast between the two brains in point of vascularity, both on the surface and the interior, was most striking. In the one scarcely the trace of a blood-vessel was to be seen; in the other every vessel was turgid with blood." (p. 13.)

Dr. Burrows has appended coloured drawings of the post-mortem appearances in the brains of the two rabbits, and they are certainly in striking contrast. He observes, however, that the brains of sheep slaughtered by butchers are much less depleted than the brains of rabbits killed in the mode he describes. This difference is owing, he thinks, to a difference in the mode of death. Sheep, he observes, die partly from hemorrhage, and partly from division of the pneumogastric nerves, and of the cervical position of the cord.

Another contradiction to the iatro-mathematical doctrine regarding the cerebral circulation is contained in a tabular synopsis of the principal pathological appearances met with in 72 insane patients of Bethlem Hospital, examined by Mr. Lawrence, and published by Dr. Webster in the twenty-first volume of the 'Medico-Chirurgical Transactions.' In 53 of the 72 cases examined, the vessels of the brain were found congested; in one only were they exsanguine—"unusually empty"—and in this case death was immediately consequent upon the hemorrhage induced by the rupture of a femoral aneurism. Dr. Burrows, therefore, observes:

> "Hence it is not a fallacy, as some suppose, to assert that bleeding diminishes the actual quantity of blood in the cerebral vessels. By abstraction of blood we not only diminish the momentum of blood in the cerebral arteries, and the quantity supplied to the brain in a given time, but we actually diminish the quantity of blood in those vessels. Whether the vacated space is replaced by serum, or resiliency of the cerebral substance under diminished pressure, is another question, into which I do not now enter." (p. 15.)

Can posture influence the cerebral circulation? We suspect each of our readers need only stoop to the ground to answer the question experimentally; yet the iatro-mathematical theory prevented the due reception of the results of daily experience, and apparently led Dr. Kellie into a wrong appreciation of some experiments he instituted on rabbits. As Dr. Burrows simply repeated these experiments of Dr. Kellie, we will subjoin his account:

> "On the 28th of December, 1842, two full-grown rabbits were killed by prussic acid, and while their hearts were still pulsating, the one (C, plate 3) was suspended by the ears, the other (D, plate 4) by the hind legs. They were left suspended for twenty-four hours, and, before they were taken down for examination, a tight ligature was placed round the throat of each rabbit, to prevent, as effectually as was possible, any further flow of blood to or from the head, after they were removed from their respective positions.
>
> "In the rabbit (C) the whole of the external parts of the head, the ears, the eyeballs, &c., were pallid and flaccid; the muscles of the scalp and bones of the cranium were also remarkably exsanguined. Upon opening the cranium the membranes and substance of the brain were pallid, the sinuses and other vessels were exsanguined; anæmic beyond my expectation.
>
> "In the rabbit (D) the external parts of the head, ears, eyeballs, &c. were turgid, livid, and congested; the muscles and bones of the cranium were of a dark hue, and gorged with blood, which at some parts appeared extravasated. Upon opening the cranium, the membranes and vessels were dark, and turgid with liquid blood; the superficial veins were prominent, the longitudinal and lateral sinuses were gorged with dark blood, and there was staining of the tissues, if not extravasation of blood into the membranes. The substance of the brain was uniformly dark, and congested to a remarkable extent.
>
> "Dr. Kellie asserts, but I think his experiments do not support him, that the contrast in the appearances within the heads of the two animals was but trifling. In my analogous experiments the contrast was most striking. In the one was to be seen a most complete state of anæmia of the internal as well as external parts of the cranium; in the other a most intense hyperæmia, or congestion, of the same parts; and these opposite conditions of the vascularity of the brain induced solely by posture, and the consequent gravitation of the blood.

> "If the cranium were the perfect sphere, as taught by Monro, and as subsequently entertained by Abercrombie, and other distinguished writers on the pathology of the brain, these effects on its circulation (which I have now described) ought not to have resulted from the force of gravity on the blood in the cerebral vessels." (pp. 17-18.)

Four coloured plates illustrate this account; and certainly the contrast is as striking as in the experiments described previously.

These researches of Dr. Burrows ought, we think, to be considered conclusive on the question, so far as experiments of the kind can be considered conclusive.

It appears to us, however, that the original theory has had much more importance attached to it than it deserves. In the first place the cranial contents cannot be considered as free from atmospheric pressure any more than the heart, liver, &c. The cranium is *not* a perfect sphere, while the sinuses of the dura mater, situate within it, communicate with the exterior of the skull by several large veins. Then, if it be granted that the skull is a perfect sphere, and the amount of fluids must at all times be equal, the necessary equalization is attained by the pressure of the columns of blood forced in by the heart, which columns, dividing and sub-dividing until, at length, lost in the capillaries, exercise in the meanwhile a compressing power upon the contractile tunics of the vessels, and when this is overcome, upon the surrounding nervous tissue. This complicated structure of vessels and nervous fibrils cannot, we think, be properly compared with a homogeneous fluid, nor can the laws of hydraulics be applied to it. In short, we think that the cranium cannot be rightly likened to a beer-barrel, nor its contents to beer; nor do we think it necessarily follows that, because the brain is incompressible, its functions may not be destroyed by compression; indeed, we believe this is acknowledged by the spheroidal theorists. The pressure, they allow, may be so great as to rupture the vessels, whether that pressure be induced by retardation of the blood in the venous circulation, or by increased energy of the arterial circulation. That the quantity of *fluids* within the cranium generally cannot be sensibly increased, is probable nevertheless; but this is not, in truth, the prominent and fundamental proposition of the iatro-mathematicians. It was, really, that the quantity of *blood* could not be sensibly increased; the other proposition was only secondary to this. The one had a great therapeutic importance, because it bore on the propriety of bleeding and other points in the treatment of apoplexy, and was, therefore, adopted exclusively by those writers whose opinions Dr. Burrows combats; the other, having no practical value, was not noticed. But, although we believe the iatro-mathematical theory never received the full consent of the profession, or was generally acted upon in the treatment of congestive affections of the head, yet it threw an air of doubt and obscurity about an important point in physiology, which has materially tended to render the progress of cerebral pathology slow and imperfect. By the removal of a great error, Dr. Burrows has done as good service to the medical commonwealth as if he had discovered a great truth. We here, however, subjoin an important caution given by him:

> "It may now be affirmed that the encephalon is not exempt from this law in physics—the gravitation of the fluids to the lowest parts of the corpse. The dis-

covery of the operation of this force on the blood within the cranium after death suggests a precaution very essential to be followed, when it is desired to ascertain the precise amount of congestion of the cerebral vessels at the time of death. In such cases a ligature should be placed round the throat of the corpse, and drawn sufficiently tight to compress the cervical vessels, and arrest all flow of blood through them. This precaution will be most required in the examination of bodies where, from the kind of death, the blood may be suspected to remain fluid in the heart and great blood-vessels. The depending or elevated position of the head, during the examination of the body, will not then induce deceptive appearances, which mislead us in our conclusions as to the previous amount of congestion in the cerebral vessels." (p. 20.)

This caution is rendered the more necessary, because the non-congested state of the brain which has been observed in persons who have been hung, or strangled, has been adduced (vide Dr. Kellie's paper) in proof of the iatro-mathematical proposition. But Dr. Burrows shows that the brain may or may not be congested according to circumstances, and these circumstances consist in the mode in which the strangulation is effected, and in the anatomy of the cerebral vessels. He thus accounts for the occasional absence of cerebral congestion in those who have suffered death by hanging:

"When criminals are hung by the executioner, the knot of the rope is usually adjusted on one side of the neck; and it is found, after death, beneath the ear, resting on the mastoid process. It has been often observed, in the dissection of such criminals, that the cheek and integuments on this same side of the head are not near so livid and congested as on the other side. The pressure of the rope has not completely obstructed the return of blood through the external jugular vein on the one side, although it has effectually stopped the current on the other. In such cases it is probable that the deep-seated internal jugular vein on the one side has only been partially compressed, and has permitted to a certain extent the return of blood from the internal parts of the cranium.

"But there is another still more efficient cause of this occasional absence of congestion of the cerebral vessels after death by hanging: it is the subsidence of the *fluid* blood after death, while the body is yet suspended, through the cervical vessels which are not completely obliterated by the pressure of the cord. And it should be recollected that there are some channels which are scarcely, if at all, affected by the compression of the rope. These other channels are the vertebral sinuses and spinal plexus of veins, so ably delineated by M. Breschet." (pp. 25-6.)

The sinuses of the cranium may also be drained otherwise than through the vertebral sinuses. In examining the bodies of those who have died by strangulation, the great vessels of the neck are usually cut across to get at the thoracic viscera; and then, when the head is elevated to open the skull, the blood gravitates, and flows from the cut ends, and the blood-vessels (previously congested) are rendered comparatively empty. Dr. Burrows further observes, in proof of this explanation, that in other instances where life has been destroyed by obstruction of the respiration, congestion is usually present.

The cerebral arteries and veins were supposed by Dr. Kellie and his disciples to stand in such relation to each other that, although the quantity of blood in the one might be diminished, it would be proportionately increased in the other. In states of anæmia, for example, a diminished quantity of blood might pass into the cerebral arteries, but a proportionate increase would take place in the cerebral veins and sinuses; and so the whole volume of blood within the skull would remain the same. Now there is no proof that such a relative condition of the two classes of vessels exists. Dr. Burrows very ingeniously shows, too, that the extravascular serum contained in the encephalon plays an important part in equalizing the pressure and the contents of the cranium. This point is, however, discussed in the next section. We need only observe, further, that to strengthen his case Dr. Burrows adduces the recorded cases of pathologists to prove that the brain is liable to be congested, loaded with blood, &c.; so that the quantity of the latter is a variable quantity.

On vascular pressure within the cranium. After objecting to some theoretical views regarding the incompressibility of the brain, Dr. Burrows proceeds to observe that the force which is impressed on the cerebral substance, through the momentum of the blood in the cranial vessels, is derived partly from the left ventricle of the heart, and partly from the reflux of the venous blood during expiration. When the brain is exposed in the living animal, the dura mater is found to rise and fall a little alternately with every systole and diastole; and experiments show that this alternate rise and fall is really (as it appears to be) dependent upon cardiac action. As to the venous reflux, we quote Dr. Burrows's lucid statement:

> "If the dura mater be exposed to view, and observed during the period of expiration, when the free return of venous blood from the brain is impeded, and a larger quantity of arterial blood is distributed with increased force towards that and other organs, the surface of the membrane is seen to rise; the brain itself swells and becomes turgid, but again subsides with the succeeding inspiration. All physiologists who have considered this subject agree in regarding these latter respiratory movements of the brain as partly, if not wholly, attributable to the reflux of the blood in the veins during expiration. Ecker, indeed, attributes these movements of the brain in great part to the ascent of the cerebro-spinal fluid during expiration. The last-mentioned writer has detailed numerous experiments, which show that if ligatures be placed upon the two carotid arteries of an animal, these cerebral movements accompanying respiration are not suspended; but if the external jugular veins in dogs (and these are the principal cerebral veins in these animals) be tied, these respiratory movements are much weakened, but not altogether destroyed; and if the cervical vessels, both arterial and venous, be divided, all movements of the vein are immediately lessened; and when the loss of blood becomes excessive, they cease altogether, and there follows a remarkable shrinking of the organ. But the effects of this reflux of the blood in the veins upon the cerebral substance are still more manifest after those accidents where, with loss of a portion of the bones of the cranium, there is also a laceration of the dura mater. In such cases the distending forces of the vessels acting on the cerebral substance are so strongly exhibited, that not only is the alternate rising and sinking of the exposed surface observed to correspond with each expiration and inspiration, but portions of the substance of the brain are actually protruded through the opening in the dura mater and bones of the cranium. Hernia of the convolutions of the brain is thus effected by a force from within, just as hernia of the convolutions of the intestines is produced by analogous forces, when the walls of the abdomen are accidentally perforated" (pp. 46-7.)

Such being the causes of cerebral pressure in continual action, it remains to ascertain what provisions have been made to equalise this pressure, so liable to vary greatly in amount, and acting on so delicate an organization as the brain, confined in its unyielding bony case. This is to be found in the cephalo-rachidian fluid. We have already, in our

Fourteenth Volume, given a *précis* of Majendie's views respecting this fluid. From experiments instituted by Ecker, we learn that the spinal theca rises and falls like the brain, concurrently with expiration and inspiration. If the theca be carefully divided, so as to leave the arachnoid membrane intact, the latter appears at the opening like a fine bladder containing fluid. This fluid is the cephalo-rachidian, and is manifestly subject to alternate increase and diminution of pressure, concurrently with the respiratory acts. It is engaged in a perpetual alternation of ascent and descent, and presses against the solid parietes with a force equal to the internal momentum. Dr. Burrows thus states his views of its functions:

> "This extra-vascular serum appears to me to be supplemental to the other contents of the cranium; it is removeable by pressure or absorption; at one time giving place to an increased quantity of blood in the cranium, at another, making up for a deficiency of blood in the vessels of the head. This extra-vascular serum not merely acts as supplemental to the varying quantity of blood, but also to the variable quantity of nervous matter in the brain. Its quantity is in the inverse proportion to the quantity of this nervous matter. Thus, in hypertrophy of the brain, there is a most remarkable deficiency of serum within the cranium; the brain, its ventricles and membranes, are so devoid of this fluid, that they are almost dry; on the contrary, in atrophy of the organ, the ventricles and membranes are distended with fluid.
>
> "This extra-vascular fluid may probably perform another office: perhaps through this cerebro-spinal fluid a more equable pressure is diffused over the whole mass of the brain and cord; and, for the reception of this regulation of pressure, may be the contrivance of those cavities called ventricles, which dip into the central parts of the brain. The sac of the arachnoid may subserve the same purpose." (p. 56.)

This theory is plausible, and will certainly serve to explain some obscure points in cerebral pathology, but we apprehend its plausibility will prevent a ready adoption of it by pathologists, without corroboration by further experiments and observations. Essentially, it considers the spinal column and cranium as one cavity, and consequently reference must be had for the future to the functional condition of the spinal cord in all cases of cerebral compression. The amount of the cephalo-rachidian fluid must exercise, if this theory be correct, a most important influence on the functions of the cerebro-spinal axis. The means and agencies by which that amount may be diminished and increased, will also have an important bearing on the pathology and therapeutics of the brain and spinal cord. Dr. Burrows details a case of chronic inflammation of the investing membranes of the brain, with copious serous effusion, and which latter, in his opinion, by varying in site and amount, was the cause of a peculiar variation of the symptoms. The patient presented "a remarkable alternation between excitement and calm, between returning power and paralysis. On one day she had no command over the sphincters, and the lower limbs appeared almost powerless and insensible, but exhibiting slight involuntary movements when the feet were pricked. On other days she called for the bed-pan, and the legs were immediately drawn away with an expression of pain upon a slight amount of irritation." We think this case extremely inconclusive, since other causes might have been in operation, as, for example, recurring alternations of vascular action and atony.

It is certain, indeed, that the pathological influence of this fluid must always be difficult to appreciate, until its physiological relations and behaviour are better understood. There are some anomalous cases of paraplegia which may ultimately be elucidated by this theory; we allude to those examples in which there is no perceptible disease, either of the investments or structure of the spinal cord, and in which, upon some vivid emotion occurring, the paraplegia temporarily disappears. Now it *may* so happen (we put the case confessedly as a remote hypothesis), that in such examples there is a deficiency of pressure upon the dorso-spinal cord, because of a diminished quantity of the cerebro-spinal fluid, and that when in consequence of an emotion, the brain receives a greater influx of blood, the pressure then becomes sufficient for normal action;—that, in fact, the dorso-lumbar cord in such cases is permanently in a condition like that of the brain in syncope. The following are Dr. Burrows's general deductions:

"The foregoing inquiry will elucidate many interesting phenomena observed in states of vascular congestion or depletion within the cranium. When arterial or venous congestion of the brain is suddenly induced. the first effect will be an increased pressure on all the contents of the cranium; this pressure will cause the expulsion of a portion of this extra-vascular serum into the spinal canal. On the other hand, when abstraction of blood from the cranium is effected, there is a tendency to shrinking of the encephalon, but the vacated space is immediately occupied by a certain quantity of this serum. But when the power of the heart is inordinately increased for any length of time by stimulants, general plethora, or hypertrophy of the left ventricle, a train of symptoms is remarked similar to those produced by varying degrees of mathematical pressure artificially exerted on the brain.

"These effects of increased determination of blood to the brain may be witnessed even where there is no structural lesion of that organ, although the effects are probably partly obviated by the anatomical peculiarities just described; but they will be most strikingly displayed where there is pre-existing structural disease in the cranium.

"When obstruction to the return of blood from the brain takes place, so that the blood becomes almost stagnant in the sinuses, that part of the force of the left ventricle (which, in the normal state of the cerebral circulation, is expended partly in propelling the blood onward through the capillaries towards the right auricle, and partly in distending the vessels through the cranium) is, under such circumstances, expended upon the interior surface of the cerebral blood-vessels. This pressure is partly sustained by the resistance of the vascular tissues, and the remainder is borne by the surrounding cerebral substance. Whatever this force may be, it becomes a source of increased pressure upon the cerebral substance; and the more so, according to the pre-existing morbid states of the encephalon and its vessels.

"When the circulation is thus excited or obstructed, an obvious state of congestion of the integuments of the head and face is produced, and from the experiments I have detailed, it may, I think, be inferred that a simultaneous congestion of the internal vessels of the cranium is formed.

"In previously healthy conditions of the cranium, when it contains nothing but the brain and normal quantity of serous fluid, the cerebral substance may readily accommodate itself to a temporary increase of blood in its vessels (arterial or venous, or in both), and to the consequent pressure, by expulsion of a certain amount of serum; but when the cranium contains abnormal and unremoveable substances, then the brain cannot bear these accessions of vascular fulness and consequent pressure.

"In those pathological states of the encephalon where there is an increase in the quantity of solid matter within the cranium, and a diminution of the quantity of extra-vascular serum, as in hypertrophy of the brain, tumours and cysts in that organ, and in large extravasations of blood on the surface, every cause which is capable of exciting the heart's action produces a notably increased disturbance of the functions of the brain. The variable character of the symptoms of cerebral disturbance in these permanent lesions within the cranium, are thus probably accounted for by the variable vascular pressure. Andral has offered a nearly similar explanation of the occasional recurrence and intermittent character of cerebral symptoms, although their supposed cause, organic disease in the cranium, is permanent. It seems to me probable that many permanent structural lesions within the cranium do not affect the functions of the brain by pressure, except when there is some cause in operation capable of inducing vascular congestion, or when the lesion is of a mechanical nature, or is gradually increasing." (pp. 58-60.)

Dr. Burrows thinks it necessary to combat some views lately published, in which syncope is represented as being allied to apoplexy, and dependent on venous congestion. He maintains that it results from insufficient vascular pressure, and not from the inadequate supply of blood to the brain. In syncope arising from strong moral emotion, the quantity and quality of the blood, he argues, are unaltered previously to the attack, and the suspension of the cerebral functions arises solely from the diminished energy of the heart. We apprehend that these views of Dr. Burrows will not meet with more general acceptance than those he combats. It may be justly argued, we think, that if the heart be so enfeebled as not to be able to propel the column of blood with the force necessary to cerebral action, it will propel a column of diminished size, or in other words, in syncope, the heart's action being enfeebled, the contractility of the arteries will react upon the column of blood, and by rendering it smaller, diminish the supply of blood to the brain, as is generally supposed. Now the importance of a due supply of blood is, we think, sufficiently manifest from the great vascularity of the gray substance. Dr. Burrows argues, that in cases of hypertrophy of the brain, the substance of the organ is extremely anæmic, for the hypertrophy, by filling the skull, completely prevents the blood entering the capillaries. Dr. Burrows cannot wish his readers to understand that the *quality* of the blood has not quite as much influence on the functions of the brain as the quantity. In cases of anæmia from hemorrhage or defective sanguification, it does not appear probable that the amount of the fluid circulating through the vascular system is diminished, nor is it improbable that the increased rapidity of the heart's action in a great degree counterbalances the diminished force of the systole. Besides, we have examples of cardiac affection, unaccompanied by an anæmic condition of the blood (meaning by anæmic, a deficiency of the red globules), in which the systole is slow and feeble, and yet there are few of the characteristic symptoms of anæmia. There can be no doubt that the horizontal position increases the vascular pressure on the brain, when the heart's action is enfeebled, but it also increases the quantity of blood passed through the brain in a given time. We would ask also, if we grant (as we willingly do) that a certain amount of vascular pressure is favorable to the functional activity of the brain, how is it that this acts? Is it not by favoring the circulation through the numerous capillaries, and thus increasing the assimilating activity of the cerebral substance? And, if so, may not the proper condition of the matter to be assimilated (the blood) have as important an influence on the organ as the proper condition of the vessels that convey it; or the organs that assimilate it? We think when Dr. Burrows inquires how it is that diminished vascular pressure interrupts cerebral function, he will be induced to modify his views in some degree.

Cerebral disorders from ligature of the carotids. It has long been known that compression or ligature of the carotid arteries has an important influence upon the condition of the brain. A female was operated upon by Mr. Key for aneurism of the innominata.

> "In about an hour and a half after the operation she appeared asleep, the respiration being natural, with the exception of a snore. This noise gradually became fainter, and she expired in about two hours more; just four hours after the operation. Upon examining the body it was found that the opening of the other (left) carotid from the arch of the aorta was nearly closed; it was scarcely one tenth its natural size. The substance of the brain was healthy; its vessels sound, and containing the ordinary quantity of blood. There was a little serous effusion between the membranes."

Now, Dr. Burrows, (a little influenced by his *idolum specûs*, we think) observes that "there appears nothing to account for this woman's death but the sudden diminution of the momentum of the blood in the cerebral arteries, occasioned by the closure of the only pervious carotid." But surely a defective *supply* of blood to the brain, in this case, so much below the ordinary quantity sent to it, may be allowed to go for something, and that the want of blood, as well as the want of pressure, was a cause of death. In cases in which one carotid has been tied, the cerebral disorder has been characterized by hemiplegia of the side opposite to the obstructed artery, with disorganization of the hemisphere of the same side. These results may, in part at least, be very fairly attributed to defective nutrition of the hemisphere implicated; but Dr. Burrows, faithful to his theory, says that the cause of the commencing hemiplegia is the pressure induced on it by the vascular distension of the opposte hemisphere. There is no proof, however, that one hemisphere can thus compress the other; but, on the contrary, it is probable that the falx cerebri will effectually prevent such a result, just as the tentorium protects the cerebellum from compression by the cerebrum.

Considering that compression and ligature of the carotids have been found useful in epilepsy, mania, and other chief-diseases, Dr. Burrows thinks that the ligature may be resorted to in hopeless cases of the kind alluded to.

> "Although the ligature of the common carotid is attended with risk to life in some cases (perhaps in the proportion of one death in four operations), still experience proves that where proper precautions have been taken the operation is not so dangerous as many suppose. Therefore, in violent and hopeless cases of epilepsy and some kindred maladies, which are characterised by extreme cerebral congestion, it appears to me that, other remedies failing, this operation may be fairly resorted to. I am aware of the responsibility of advocating a remedy attended with risk to life; but are not all our best remedies most violent poisons in the hands of the unskilful? But this truth does not forbid their use by the more expert. So may this powerful method of influencing the cerebral circulation be justifiable in aggravated cases of the class referred to, and where the pre-

cept of Celsus, '*satius est anceps remedium experiri quàm nullum*,' may be fairly put into practice." (pp. 78-9.)

We would only observe that there is another good precept to be remembered, and that is "Saltem non nocere."

Causes of coma. The progress of neurology, during late years, is amply marked by the clearer comprehension of the phenomena of cerebral diseases. What is the condition of the brain that produces coma? was a question not easily answered twenty years ago; and the answer involved the hypothesis of an obstruction to the flow of the "nervous fluid," or a confession of ignorance. The difficulty of explaining this condition was increased by the circumstance that often no appreciable change in the constitution, or vascular system of the brain, could be detected after death from profound apoplexy. Dr. Abercrombie has particularly noticed examples in which not even congestion of the brain was observed. Dr. Burrows's explanation is, that the active depletion practised before death, in two of the cases mentioned, had emptied the cerebral vessels, and that the apoplexy was immediately dependent on sudden vascular pressure. Now although bloodletting may relieve this, the respiration has already become involuntary, slow, and stertorous, and the blood decarbonised; so that the coma is kept up by the circulation of the latter through the brain, and the patient actually dies of slow suffocation. It is probable that in deaths from acute laryngitis, when the respiration has been restored by tracheotomy, and in deaths after resuscitation from hanging, drowning, and suffocation from irrespirable gases, there is the same fatal condition of the brain.

Is the coma in the so-called serous apoplexy dependent on pressure from serous effusion, or on vascular pressure from congestion? Dr. Burrows argues in favour of the latter. He thinks the diagnostic symptoms between serous and sanguineous apoplexy are more fanciful than real; that, in fact, the serous effusion is itself a sequela of vascular congestion, and that the serous apoplexy is no other than simple apoplexy, that is to say, cerebral congestion terminating in effusion.

From our review of Dr. Neisser's work (see the preceding article), it will, we think, be clearly manifest that serous effusion is one of the modes in which the subacute form of arachnitis terminates. In such a form of arachnitis there is doubtless vascular congestion; but we cannot conclude with Dr. Burrows that that congestion is the cause of the coma and other signs of a fatal termination. We have no evidence of "vascular pressure" during the earlier stages of the disease, when the vascular action is the greatest. On the contrary, there is an increased activity of the cerebral functions. It is only when the serum accumulates within the cranium, and especially at the base of the brain, that the comatose symptoms come on (vide p. 405); for it must be remembered that the seat of the effusion has an important influence on the development of symptoms of pressure.

Dr. Burrows applies similar views to extravasation, and advances that the effused blood is not the cause of the apoplectic coma. We will quote his opinions on this point:

"I am of opinion that apoplectic coma is rarely dependent upon the extravasation of blood, although the concomitant paralysis undoubtedly is. Upon the examination of the brains of apoplectic patients we sometimes find large extravasations of blood, which from peculiar appearances in the clot, we feel assured have existed there for many days, or even weeks, and yet during the greater part of that period there has been no coma. Upon other occasions we discover small extravasations of blood within the brain, which, from their appearance, we can decide have only been effused a short time prior to death; and, nevertheless, there has been a well-marked coma in these cases. Hence, if pressure be regarded as the physical cause of apoplectic coma, and that pressure is supposed to be occasioned by the extravasated blood, then we must account for the parodox of a small extravasation producing a coma which terminates fatally, and a large effusion of blood having no such effect." (p. 92.)

It appears to us that Dr. Burrows is wanting in his usual lucidity and precision here. There appears somewhat of special pleading in the argument. The observation we have just made respecting the importance of referring to the exact locality of the extra-vascular causes of pressure applies with peculiar force to extravasation of blood. In the two cases related by Dr. Burrows in proof of his views, the influence of a hemorrhagic site is very clearly shown. We have already observed in the article referred to that the effects of pressure from extra-vascular causes are manifest in proportion as the locality of the compressing cause is near the base of the brain. Now in the case of Mrs. G. (detailed by Dr. Burrows) the first extravasation of blood was "a large clot, probably an ounce, effused into the anterior half of each anterior lobe;" and the symptoms when this occurred are thus described:

"In the forenoon of Sept 12th she had complained of vertigo and drowsiness while engaged in her domestic affairs. She did not fall or lose her consciousness, but contrived to get up stairs to her bed room. She was assisted to her bed, where she remained perfectly sensible for some hours, but stupor increased; so that, towards evening, she could not be roused at all. Her family now became alarmed, and sent for medical assistance. She was found lying on her back, perfectly insensible; her face pale, and expression placid; hands cold; breathing natural; pulse 120, small, and feeble; pupils dilated, and insensible to the influence of light."

On the following day she was restless and delirious; so much so, indeed, that she could hardly be kept in bed—a state of things quite unlike apoplectic coma. The second extravasation, occurring about forty-eight hours before death, was beneath the cerebral arachnoid over the left hemisphere, and at the base of the brain; all the ventricles, although not distended, contained clots of soft and uniformly black blood. There was also a considerable quantity of blood effused into the tissue of the spinal pia mater, especially over the medulla portion. This blood, Dr. Burrows thinks, was probably derived from the ventricles of the brain, it having followed the course of the cephalo-rachidian fluid. The symptoms accompanying this extravasation are thus described: "Her countenance was haggard; her eyes fixed and vacant; she was roused with difficulty; her articulation imperfect, and she answered incoherently. She gradually became comatose, and sank on the third day after her admission." Here we have a state of things very different to the preceding, with a corresponding difference in the seat of the extravasation. Of course this solitary case would prove nothing; but as it is corroborated by numerous similar cases, we must fairly presume that the difference in the seat of the effusion was connected with the difference in the symptoms.

The next case is equally instructive. A female had a series of fits. The last and fatal fit was profoundly apoplectic, with coma, stertor, and death in twelve hours. On examining the corpse there was a clot of recently effused, dark-coloured blood, extending nearly across the tuber annulare. The penultimate fit (six weeks previously to the fatal attack) was complicated with hemiplegia of the right (?) side. The corresponding post-mortem appearance was a diffused yellow staining in the substance of the right hemisphere, becoming deeper downwards, and a large and somewhat hard clot of blood external to the lateral ventricle, a softening or breaking down of the corresponding corpus striatum, and part of the optic thalamus.

The extravasation may also influence the development of coma, not simply by its immediate relation to important structures, but by its relation to the cephalo-rachidian-fluid. The pressure of extravasation spread over the hemispheres will be expended upon the whole depth of the brain before it can act upon the base, and, in particular, the cephalo-rachidian fluid in the ventricles will be interposed like a water-cushion between the two surfaces. Should the extravasation be gradual, a corresponding absorption of this fluid will tend to equalize the pressure, and the symptoms may be comparatively trifling; for it is well known how extensively the hemispheres may be injured without serious consequences. But if the blood be extravasated at the base, it is in immediate and direct contact with the great trunk conductors to and from the seat of perception and intellect, and, by compressing them, or otherwise interrupting their functions, will interrupt the functions of the superincumbent mass.

The connexion between apoplexy and diseases of the heart. Having arrived at sounder physiological views, Dr. Burrows next proceeds to apply them to pathology. The state of the circulation through the brain must necessarily be influenced by all disorders of the heart—of that central force-pump by which the circulation throughout the system is maintained. Portal was the first writer who devoted any considerable attention to the connexion between apoplexy and cardiac diseases. Dr. Kellie's experiments and inferences probably prevented Portal's doctrines taking that root in the professional mind which might have been expected; and the omission of Dr. Abercrombie to note specially the action of a diseased heart on the cerebral circulation, in his investigations into the pathology of apoplectic affections, tended to keep them still further in the background. But if error multiplies itself, truth is never lost; and Portal's views began at last to modify the views of English pathologists. Of late years we think the doctrines as to the influence of cardiac disease on the vascular system within the cranium have been much less undecided than Dr. Burrows seems to suppose. Dr. Kellie's doctrines never had an extensive influence in *practice;* and so soon as Bright, Craigie, Brichteau, Bouillaud, Andral, Hope, &c., took up its pathology, the influence of cardiac disease in predisposing at least to apoplexy was clearly established. "It is found, for instance," Dr. Craigie observes, "that in most cases of apoplectic seizure, either the left ventricle of the heart is affected with hypertrophy or dilatation, or the mitral valve is ossified, and its aperture contracted; or the aortic valves are more or less diseased; or even the whole of the arch and part of the descending aorta may be ossified, roughened internally, and incapable of transmitting the blood properly."* Nevertheless, it is interesting to find that Dr. Burrows presents us with more precise data, founded upon tabulation by the numerical method. In the subjoined table he gives an analysis of 132 cases of apoplexy and sudden hemiplegia, derived from trustworthy sources, arranged with reference to the coexistence of cardiac disease:

" Authors.	Cases.	Diseased Heart.	Per Cent.
Andral	25	15	60·
Clendinning	28	15	53·5
Hope	39	27	69·4
Burrows	34	23	67·6
Guillemin	6	4	66·6
Total	132	84	63·6

"The inference from the foregoing calculation is, that in any given number of cases of apoplexy and sudden hemiplegia, no less than three fifths will present unequivocal signs of cardiac disease: either hypertrophy, dilatation, valvular disease, or some combination of these lesions. This proportion proves that the frequency is much greater than is commonly supposed, even by those who admit the occasional influence of cardiac disease in the production of apoplexy and hemiplegia." (p. 117.)

The order in which the pathological phenomena arise in cases of this kind is very neatly and lucidly stated by Dr. Burrows. The constantly augmented force with which the blood is projected from the hypertrophied left ventricle gradually dilates the cerebral arteries, and diminishes their contractile power. The surrounding cerebral substance thus becomes more liable to pressure from the column of blood sent through them, and on any sudden increase of this pressure coma may take place, or the tunic of an artery yield, and hemorrhage occur. Should there be an atheromatous condition of the arterial tunics, the latter is the more likely to take place. Valvular diseases of the left side of the heart act on the brain by exciting congestion, first of the pulmonary vessels, then of the right cavities of the heart, and, next, of the venous system within the cranium. While the blood is thus retained on the one side, if there be also hypertrophy of the left ventricle, it is propelled with increased force on the other; and thus the brain is included between two opposing forces, and both classes of vessels distended and dilated.

"The relative frequency of these several cardiac lesions in cases of apoplexy and sudden hemiplegia may be estimated from the following analysis of twenty-five cases recorded by Andral, and thirty-four cases taken from my own case-books:

	No. of Cases.	Heart diseased.	Hypertrophy with valvular disease.	Hypertrophy (simple).	Valvular disease.
Andral . . .	25	15	9	4	2
Burrows . .	34	23†	16	6	6
Total . .	59	38	19	10	8

(p. 123.)

Influence of age on apoplexy. In considering cardiac disease as a cause of apoplexy and hemiplegia, we have followed our author hitherto through his researches exclusively devoted to a consideration of the force-pump. But the condition of the conducting tubes merit, we think, an equally decided notice. On this point Dr. Burrows's work is defective, as he only incidentally alludes to the condition of the cerebral arteries in cases of apoplexy. It has occurred to every practitioner to observe that persons suffer from extensive disease of the heart, and yet very rarely experience an apoplectic attack, or any important symptoms which can be referred to the brain; and, upon further consideration, they will probably remember that almost all the cases of prolonged cardiac disease, thus free from cerebral complication, have occurred in young persons. We know of no numerical data which show the number of apoplexies in persons under twenty years of age; but it is remarkable that they are so few as to be thought unworthy of notice by Dr. Burrows and other numerical writers. In an elaborate table which Dr. Burrows has drawn up of the ages at which 215 cases of apoplexy and hemiplegia occurred, the cases being related from his own records, or from those of trustworthy observers, there is not one entered under the age of twenty. It is a circumstance worthy notice, too, that the numbers at each age are progressively greater as the age advances. We subjoin the very valuable table which Dr. Burrows has made; in which the proportion to the population living at each age is indicated—a most important estimate:

---

* Elements of the Practice of Physic, 1840, vol. ii, p. 885 See also the Edin. Med. and Surg. Journ., vol. xix.

† In one case there was simple dilatation of the cavities.

Age.	No. of Cases.	Population of this age.	Proportion of cases in 1000 persons.
20 to 30	16	3,000	5 3
30 to 40	30	2,500	12·0
40 to 50	40	1,800	22·2
50 to 60	41	1,300	31·5
60 to 70	54	1,000	54·0
70 to 80	30	500	60·0
80 and upwards	4	200	
Total	215	10,300	

"It must be remembered that the figures in the second column of the preceding table do not represent the *actual* numbers of cases of apoplexy and hemiplegia occurring at successive ages in any given population (ex. gr. 20,000), but only the *relative proportion* of cases in each successive decade, and this is compared with the numbers living of the same age. The population is assumed to be 20,000, of whom about one half will have attained the age of twenty years; and the numbers living in the successive decennial periods will be *nearly* those assigned in the third column of the table." (p. 131.)

Dr. Burrows, adopting some views of Dr. Clendenning as to the increase in size and force of the heart, with increasing age, attributes this greater frequency of apoplectic affections at the more advanced ages to the increased force and size. Bizot found that the walls of the left ventricle, and the septum, increase in thickness as age advances; but he found also, that a gradual change in the arterial tunics was equally constant. Baillie, Stevens, &c., have remarked how constantly they are affected in old age by atheromatous degeneration, and, indeed, considered it almost a normal state. Bizot found opacity of the membraneous portion of the semilunar valves in 80 per cent. of females aged from 60 to 89 years; and in 92 per cent. of men of the same age. The first traces of this change are usually observed in men about the age of 49. Considering, then, the constancy of atheromatous degeneration in persons aged about 50, we should feel inclined to think that the great predisposing cause of apoplexy at ages above that is this brittle condition of the arteries, whereby they are rendered unable to resist—not the column of blood impelled ordinarily by the hypertrophied left ventricle, but the accidental propulsion, or retention, of a larger quantity of blood than usual. In short, that the adaptive power existing in the resiliency of the arteries of youth and manhood is impaired in proportion as age advances; and that, consequently, in cases of apoplexy, we should look as carefully to the condition of the conducting tubes—the cerebral vessels, as to that of the central force-pump—the heart. Whether, however, the predisposing cause be in the one part of the circulating system, or in the other, or whether it be in both, the quotation from Goldsmith, by which Dr. Burrows illustrates the most suitable treatment, is not less apt:

"To husband out life's taper at the close,
And keep the frame from wasting by *repose*."

Treatment of Apoplexy. The practical deductions as to treatment, as given by Dr. Burrows, seem to us to be eminently sound and valuable, so far as they go. In the first place, the posture of the patient should be attended to; he should be so placed that the blood may gravitate from the head. Secondly, however valuable the indication by the pulse may be as to the propriety of bleeding, that operation should not be decided upon until the condition of the heart has been ascertained. Without this knowledge the pulse cannot be taken as a safe guide.

> "But suppose a careful examination of the apoplectic or hemiplegic patient's heart discloses the existence of valvular disease, to the extent of obstructing the circulation through its cavities, here the pulse will be a most deceptive guide as to the propriety or impropriety of abstraction of blood. If the mitral valve be principally implicated, and allow of regurgitation from the left ventricle, the small and irregular pulse so commonly observed with that lesion would probably dissuade from that free abstraction of blood which the cerebral symptoms might require. If, in another case of apoplexy or hemiplegia, the aortic valves be found diseased to the extent of not only obstructing the onward current of blood, but also of allowing regurgitation into the ventricles, during its diastole, there will probably be associated with this lesion considerable hypertrophy of the left ventricle. Here will be observed a full and vibrating or thrilling pulse, but a pulse of increased action without real power, and hence a deceptive pulse; and one which, if it be regarded without reference to the structural changes of the heart, would invite to a more copious abstraction of blood than was called for by the general symptoms. In each of these last-mentioned cases, greater relief to the symptoms will be obtained by a free local abstraction of blood from the vicinity of the heart (either by cupping from beneath the left mamma, or between the left scapula and spine,) than by a much larger depletion by venesection." (p. 142.)

When the inflammatory stage supervenes, the symptoms

> "May generally be controlled in a most striking manner by small local depletion from the temple or mastoid process on the side opposite to the paralysis; by the application of cold to the head, and by the administration of purgatives, by restricted diet, and by extreme quiet in the sick room. In addition to these remedies I have found, when the heat of head is diminished, that a blister ap-

> plied near the occiput affords great relief to the oppressive headache. If the patient be not very advanced in years, or extremely exhausted by depletion, great benefit will be derived at this stage of apoplexy from the administration of small doses of mercury. One grain of calomel may be given every six hours, leaving the mineral to act as a purgative, or to slightly affect the gums, but not allowing the mercury to produce ptyalism, which is generally very distressing to the hemiplegic patient, whose powers of mastication and deglutition are already impaired by his disease." (p. 146.)

The sequelæ of an apoplectic fit require great attention. The pains in the palsied extremity, dependent most probably on centric irritation, are only relieved by time. The changes in the brain gradually advance, and the affected parts at length put on a more healthy action. It is during the convalescent stage that the greatest circumspection is required, as patients are apt to resume their mental and bodily activity at a very early stage. If this be permitted, Dr. Burrows observes—

> "One of three accidents may happen. First, if there be too early excitement of the cerebral circulation after the fit, and before the fluid parts of the extravasated blood are absorbed, and while the cerebral substance around is still soft from infiltration, and before a cyst is formed around the clot, a fresh extravasation will take place, and produce irreparable mischief.
>
> "A second accident likely to ensue from too early exertion after apoplexy, followed or not by palsy, is a renewal of the head symptoms, and subsequent disorganization of the cerebral substance.
>
> "A third serious consequence of a too early return to mental occupation, or the anxieties of business, is the supervention of inflammation of the brain and its membranes." (p. 149.)

So far as the circulatory system is involved, in the treatment of apoplexy, this view will, we think, be acknowledged to be extremely judicious. The treatment, however, of this class of affections involves more important and recondite points than the dynamics of a diseased heart. The circulation through the brain is liable to be impeded by numerous other agents: Diseases of the air-passages, lungs, and liver are amongst those which act like cardiac disease. Then there is that whole class of affections in which there is morbid change in the circulating fluid, as in disease of the kidneys; and another numerous class may be found in diseases of organs which exercise an incident excitor action on the brain. Gastric disorders belong to this category. Nor can arthritic affections, so frequently seen in men of a sanguineous temperament, and so constantly connected with atheromatous changes in the arterial tissues, be overlooked.

The connexion between cardiac diseases and functional disorders of the brain. It is a necessary conclusion, from the foregoing experiments and deductions, that diseases of the heart must have important etiological relations to functional diseases of the brain. There result from the disturbance of the cerebral circulation, vertiginous affections, cephalæa, nervous irritability; and even insanity, may (with a suitable concurrence of circumstances) be immediately dependent on cardiac disease. Dr. Burrows first relates five instructive cases, to show the influence of the latter in inducing epistaxis. In one of these the hemorrhage appeared to be periodic, and the whole warn us, that it should not be regarded as an isolated or unimportant symptom. Dr. Burrows even states his opinion that it may "often be considered as strictly pathognomonic of an obstructed circulation through the heart, as hemoptysis is symptomatic of tuberculated lungs, or intestinal hemorrhage of an indurated liver." Probably that congestion of the Schneiderian membrane, which excites sternutatio—one of the apoplectic molimina—is sometimes dependent on the same causes as the epistaxis.

Headache. This is often a prominent symptom in hypertrophy of the heart, and its origin is often mistaken.

> "These persons generally have pallid faces, and peculiarly hard, incompressible pulses; they are troubled with severe and obstinate headache, vertigo, some impediment in the speech, muscular tremors, or imperfect palsy. The histories of these patients almost invariably agree in one particular—that they have been addicted to spirit-drinking; they are also liable to be attacked with profuse hemorrhages, and ultimately become affected with general dropsy. My attention was first directed to these patients by Dr. Latham, and numerous instances have I since seen; and the more closely I have studied them, the more frequently have I found such persons labouring under hypertrophy of the heart, without evidence of great valvular obstruction, but perhaps combined with albuminous urine." (p. 176.)

Nervous irritability is another affection consequent on functional disturbance of the brain, the origin of which may be traced to cardiac disease.

> "Middle-aged persons, of both sexes, in the upper ranks of society, apply occasionally for medical assistance, and who are suffering from uneasy sensations in the head, lowness of spirits, feelings of debility, occasional faintness, disposition to sigh, urgent desire for fresh air about them, irritability of temper, incapability of steady occupation, disturbed rest at nights. Such persons are often supposed to be dyspeptic, hysterical, nervous, or on the verge of insanity. Neither they themselves, nor their ordinary medical attendant, have suspected the existence of any disease of the heart. Auscultation in these cases has several times revealed to me the physical signs of valvular disease in the heart, or of serious changes in the aorta, and then, upon more close inquiry, other symptoms indicative of interruption to the course of the blood through the heart are confessed to.
>
> "The history of such individuals often informs us that they have suffered from rheumatic fever many years before, or they have had some previous severe inflammatory affection of the chest, most commonly pleurisy; or they remember, under the influence of mental excitement or bodily exertion, many years ago, having felt some peculiar sensation in the cardiac region, or that they suddenly fainted. The subsequent progress of such develops many more unequivocal symptoms of confirmed cardiac disease." (pp. 178-9.)

Dr. Burrows thinks if auscultation were generally employed among insane patients that numbers would be found to be suffering from cardiac lesions. Dr. Burrows rides his hobby pretty vigorously here. May not disease of the brain induce disease of the heart? We must not suppose the brain to be a passive organ; only acted upon, and never reacting. Cardiac diseases are certainly more frequently fatal amongst the insane than amongst the general population. In an interesting table given by Dr. Thurnam, in his "Statistics of Insanity," the deaths from diseases of the heart in the Retreat, near York, are 6·402 per cent. of the total deaths; while in England and Wales, according to the returns of the Registrar-General, they were only 1·075 per cent.

The connexion between acute cardiac disease and cerebro-spinal affections. Dr. Burrows gives a short literary history of the progress of our knowledge as to the connexion between acute diseases of the heart and spasmodic and maniacal affections. Six of these anomalous and deceptive cases have come under his own observation; so that he is led to believe they are of more frequent occurrence than is generally supposed. These and other examples from authors are detailed, the whole numbering sixteen cases of pericarditis, complicated with disease of the brain and spinal cord. Eleven of these were fatal; and Dr. Burrows observes, in not one of them could a trace of disease be discovered in the brain or its membranes, and a morbid change in the spinal cord in only two. In seven of the sixteen there was no rheumatic affection. In four of the cases treated successfully the diagnosis of cardiac disease was satisfactorily established; in the fifth it was suspected. In two of the eleven fatal cases cardiac disease was detected during life; in one it was suspected; and in the remaining eight was only discovered on examination of the corpse.

Various explanations have been attempted of the pathological connexion between functional disorder of the cerebro-spinal axis and cardiac lesions. Dr. Burrows adopts the explanation of Dr. Bright (which also closely coincides with that of Bouillaud); namely, that the inflammatory affection in these cases implicates the phrenic nerve, and that it is the more immediate means of communicating the irritation to the brain and spinal cord. Dr. Hope considered that the pneumogastric nerves might be the channel, while Dr. Watson is of opinion that the cerebral symptoms depended upon a disturbance of the cerebral circulation.

We are inclined to think, on a careful perusal of Dr. Burrows's cases, that they are too heterogeneous to be tabulated, or to form a basis for numerical deductions. The rheumatic cases should have been arranged in one division; for there is no analogy between case 1, headed "Active Articular Rheumatism, complicated with Carditis and Pericarditis, presenting the ordinary symptoms of an Inflammatory Affection of the Brain," occurring in one of the boys at Christ's Hospital, or case 13, of a young man labouring under general dropsy, and dying of apoplexy, and case 16, of a middle-aged man suffering from asthma and an extraordinary curvature of the spine. A careful analysis of such cases would, we think, show that our estimate of their value is just. In case 2 (related by Dr. Burrows) the head was not examined. In case 3, by Dr. Abercrombie, chorea and hysterical (?) delirium came on subsequently to the inflammatory attack, and were probably the result of excessive depletion; in this case, also, it does not appear that the head was examined. In cases 9 and 10, the supervention of dementia and insanity was a sufficient proof of the morbid change in the brain or membranes.

We think that the cerebral symptoms in cases of the kind described by Dr. Burrows do not arise from one or two causes only. When it can be distinctly shown that the inflammatory action in the pericardium has implicated the phrenic nerve, the theory of Bright and Bouillaud, adopted by Dr. Burrows, will suffice. But Dr. Burrows should not, we think, have omitted to point out distinctly the cases in which this nerve was implicated. In six of the cases we might *suppose* such a complication, in the remainder there is not even ground for the supposition. On the other hand, it must be remembered how many cases of pericarditis occur without this cerebro-spinal complication.

Another cause operating in the rheumatic forms of this class of cases, is a metastasis of the morbid action to the membranes of the brain and spinal cord. Dr. Burrows, without denying its occasional occurrence, objects, that in not one of the eleven cases could a trace of disease be discovered in the brain and its membranes. Dr. Burrows has himself explained how excessive congestion may exist before death, and yet be undiscovered after death. But we apprehend that it is precisely in rheumatic metastases we might reasonably expect such a result, without allowing for the effects of depletion. We see rapid metastasis from one joint to another, inducing apparently violent inflammatory action and impeding the functions of the articulation, and yet leaving no trace of its previous presence after it has disappeared. Just such a state of things may occur to the serous membrane of the brain. Further, changes may be thus induced in the delicate structure of the brain, which, inappreciable to the naked eye, would be rendered obvious by the microscope, and yet no mark of abnormal vascular action remain.

In chronic cases, the complication of renal disease with pericarditis is not unfrequent; and, as one of the principal secondary results of structural disease of the kidneys is to disorder the functions of the brain, as well as of the serous membranes generally, we think attention to those organs in cases of the kind under discussion is essentially requisite, if we would attain a perfect knowledge of their pathology. Now, we do not find that Dr. Burrows has directed his attention at all to this important point. Lastly, we think vascular pressure from the diseased heart sufficient under a suitable combination of circumstances to develope the complicating symptoms.

Treatment. Although we have differed in opinion with Dr. Burrows as to the pathology, we fully coincide with him in enforcing the necessity of an early examination of the heart by means of auscultation in all obscure and intractable affections of the brain and spinal cord. Dr. Burrows's plan of treatment is, to deplete repeatedly from the region of the heart by cupping-glasses applied between the left scapula and the spine, or by the application of leeches near the left mamma. Venesection may be indicated in robust persons, and he is not of opinion that free depletion from the arm favors a metastasis to the heart in rheumatic inflammations. As regards counter-irritants.

> "There is no remedy," Dr. Burrows observes, "upon which, after local depletion, I place more reliance, in the treatment of pericarditis, than a blister, and, in the more severe cases of pericarditis, characterized by hurried respiration, anxious countenance, great dyspnea, short cough, frequent small pulse, with the physical signs of pericardial effusion, the amendment consequent on the application of a large blister to the sternum is so rapid and striking, that it can hardly be realized by those who are not in the habit of contemplating such cases.
>
> "In all cases of pericarditis, excepting in patients affected with tubercular disease of the lungs, or in the very anæmic and cachectic, I employ mercurial preparations as freely as in acute inflammations of other parts."

He administers

> "A few large doses of calomel, varying from five to ten grains, combined with a grain of opium, and then continue half the dose of the same combination at moderate intervals, until the urgent symptoms abate, or the mouth becomes distinctly affected by the mercury."

Dr. Burrows would have written a more valuable monograph, had he cultivated a more extensive acquaintance with recent authors, both British and foreign. It is rather late in the day to quote a writer like Dr. Cooke. It is clear, too, that Dr. Burrows is warmly devoted to his *idolum specûs;* so manifest, indeed, that no harm we are sure will result from the great prominence he gives to the etiological relations of cardiac disease in affections of the brain. The work, however, taken as a whole, is excellent; it has the characteristics of an English production in an eminent degree; it is lucid, precise, practical; and will, we think, establish Dr. Burrows's reputation.

ART. X.

1. *Examen Clinique de l'Hydrotherapie.* Par H. E. SCHEDEL, Docteur en Médecine.—*Paris*, 1845.
2. *The Dangers of the Water Cure, and its Efficacy examined.* By JAMES WILSON, M.D., and JAMES M. GULLY, M.D.—*London*, 1843.
3. *The Cold Water Cure: its Uses and Misuse examined.* By HERBERT MAYO, M.D., F.R.S., formerly Surgeon at Middlesex Hospital.—*London*, 1845.
4. *A Medical Visit to Graeffenberg.* By Sir CHARLES SCUDAMORE, M.D., F.R.S.—*London*, 1843.
5. *Hydropathy.* By EDWARD JOHNSON, M.D.—*London*, 1843.
6. *Graeffenberg; or, a True Report of the Water Cure, with an Account of its Antiquity.* By ROBERT HAY GRAHAM, M.D.—*London*, 1845.
7. *Life at the Water Cure, or a Month at Malvern.* By R. J. LANE.—*London*, 1846.
8. *Confessions of a Water Patient.* By Sir E. B. LYTTON, *Bart.*—1845.

IN consequence of the modern water-cure having been originated by a non-medical and uneducated man, and having been subsequently, for the most part, adopted and professed by lay practitioners, or by medical men of somewhat equivocal reputation,—and yet more, from the system being held out as a *panacea* or cure for all diseases, with an exclusive scorn of medicinal aid,—the medical profession, as a body, have naturally enough, and not inexcusably, treated it with much contempt, not to say aversion, and have shown a pretty general determination not to admit it into the catalogue of therapeutic means. Exercising a natural influence on the public, medical men have succeeded in communicating to a large portion of the intelligent classes the feelings entertained by themselves. Thus hydropathy has become a tabooed subject, being either entirely excluded from medical journals and medical books, or only admitted into them for the purpose of being ridiculed or utterly denounced. Indeed, it is regarded almost as a violation of professional etiquette to mention this subject in the language of toleration, much more to speak of it with approbation. Accordingly, we think it not unlikely that some of our brethren, and those even of the most estimable, may regard our present article as a departure from what is medically proper, and will pronounce us almost worthy to have the severe sentence of "water-doctor" passed against us. We have, however, been too long accustomed to speak our opinions openly and boldly, when we believed them to be just, whether they were in accordance with the current notions or not, to be deterred, on the present occasion, by any apprehended risk of offending mere professional conventionalism. Whatever we conscientiously believe to be true in medical science, especially if, at the same time, calculated to promote the great end and aim of all professors of the healing art—the increase of the means of lessening the sufferings of mankind—that we shall freely and fearlessly promulgate, careless of personal consequences.

Our purpose, in this article, being carefully and calmly to investigate the real merits of the system now so widely established under the name of hydropathy, we hold ourselves absolved from mixing up this investigation with any considerations whatever respecting the merits or demerits, the objects or motives, of those who practise it. We regret to think that there is, and has been from the beginning, not a little quackery and mystification mixed up with really effective practice, in hydropathic proceedings, and that not a few of the conductors of the water-establishments have been, and are, very ill-qualified to indicate, much less to direct and conduct, any therapeutic processes capable of modifying, in an important degree, the vital conditions and functions of the human body. If it shall appear, however, as we believe it will, on further examination, that the external application of cold water is capable of being beneficially applied, in the cure of diseases, in modes of greater efficacy, and to a much greater extent, than has been hitherto practised by medical men, there remains only one course for the members of the profession to pursue, viz. *to adopt the improvements*—if such they are—regardless of their origin, or their past or present relations. When the religious reformer proposed to adapt profane airs to church psalmody, saying that he saw no good reason why the devil should have all the good tunes to himself, he is generally supposed to have acted as wisely as he thought shrewdly and spoke quaintly. In like manner, we see no good reason why the doctors of the orthodox or legitimate school, should refuse to accept good things, even at the hand of the hydropathists. They have done like things before now, as the pharmacopœia, in more pages than one, can testify; and we have not heard that there has been any great reason for regretting that they did so. For our own parts, we avow ourselves of such a catholic spirit, and so lowly-minded withal, as to be ready to grasp any proffered good in the way of HEALING, whosoever may be the offerers, and wheresoever they may have found it. Not merely hydropathy, but even mesmerism, yea, stark-naked and rampant quackery itself, may, in this sense, be a welcome knocker at the gate of physic. It is not the demerits of the donor or the birthplace of the gift, that, in such a case, we are bound to look to—but simply whether it is qualified to aid us in our glorious and divine mission of soothing the pains of our fellow-men. If it is so qualified, the baseness of its source will be lost in the glory of its use; and, if aught of its original impurity still attaches to its application in our hands, the fault will be in us, not in it. A saint may sing the devil's tunes without contamination; a hero may wield the weapon he has wrested from a robber or a murderer; the medicament or the formula of the most arrant quack may be hallowed in the prescription of the true physician.

It is in this spirit we enter upon an investigation of the claims of

Hydropathy, as propounded and practised by Preissnitz and his disciples. And we invite our readers to follow us in a like temper, convinced that they will be benefited by an examination of the subject, whether they adopt our views or not. Some of our views we are sure they must adopt—particularly this: that cold water, applied in the manner of the hydropathists, is a powerful modifier of the condition of the human body, both in health and disease, and, when weighed in the therapeutic balance with other remedies, merits, at least, a fair trial in legitimate practice.

It will be an after consideration in what manner, or under what circumstances, this trial can best be made; and, supposing the result of the trial to be satisfactory, it will be a yet further consideration, and one of great importance, how the remedy shall best be applied in the ordinary practice of medicine. We ourselves believe that distinct bathing establishments will still be found best for giving full effect to the hydropathic system, although we believe, also, that many parts of it may be adopted in ordinary practice at the patients' own homes; and the whole of it certainly be conducted at the water-establishments under the authority and general direction of the ordinary medical attendants. If hydropathy is, as we believe, a therapeutic agent of great power and value, it would be worse than absurd to exclude it from legitimate medicine; but, if it is to be adopted by the profession, it can only be adopted in a strictly professional manner. If distinct establishments are found to be requisite for its complete and successful exhibition, the members of the medical profession can, of course, sanction and patronise those only which are conducted by legally qualified and competent practitioners. And they cannot be expected to show any countenance, even to those which, although under the superintendence of legally qualified persons, are conducted on empirical or absurdly exclusive principles. A hydropathic establishment should be simply a great bathing establishment, or water hospital, and should contain the means for using water in all its medicinal forms, hot as well as cold, in the form of vapour as well as liquid, medicated as well as pure. In such an hospital, although drugs would, doubtless be but in slight requisition, it would be contrary to all rational proceedings to exclude their use entirely. The very fact of a case being sent to such an hospital, presupposes the previous failure of drugs, or, at least, presumes their unsuitableness in that particular instance; and they would, for the most part, be dispensed with in the *commencement* of the treatment, at least: but no unprejudiced and competent observer can assert that drugs should be entirely banished from the treatment of any case at all times. The same scientific judgment and the same practical skill that prescribed the water-treatment as best calculated to fulfil the indications present at any one time, could alone determine whether, at any other time, medicaments might be proper, either as auxiliaries or substitutes. Nothing but the blindest dogmatism or the wildest empiricism could maintain that, because the water-treatment is found useful, all other means must be useless; or, reversely, that, because drugs are often found beneficial, that therefore all other kinds of treatment, hydropathy included, must be injurious. The absolute exclusionist, be he water-doctor or drug-doctor, is equally unreasonable and equally unjustifiable.

In the composition of the following article we have derived our materials mainly from the published writings of hydropathists, but also, partly from personal observation of the practice of hydropathy itself, and from the reports of patients who had been the subjects of it. We have been careful to select as our anthorities the best informed and most impartial of the writers on the subject of the water-cure, and we have used our best endeavours to appropriate what alone seemed trustworthy. It is so extremely difficult for a writer, on any one side of a question that has become the subject of active controversy, to avoid partiality in relating events and drawing inferences, that we make no apology to our authors for having on many occasions refused their evidence and rejected ther conclusions. Many things, however, we have admitted on the authority of the writers alone, when they did not seem to be contradicted by other facts, and were in accordance with the general principles of physiology and therapeutics. We have so far admitted the validity of the maxim—*cuilibet in suâ arte credendum;* and, so qualified, we think the propriety of the admission will not be gainsaid. But we have gone further than this. We have accepted at the hands of our hydropathic authors more than one alleged fact and explanation, even although their validity seems to us questionable. And we have done this, because the statements are of a kind justly to challenge attention, and to demand thorough investigation.

On the whole, then, we wish the reader to be prepared to find in the following article, not simply an exposition of the doctrines of hydropathy, as they appear to ourselves well established, but such also as they are laid down by the best authorities of the water-school; one of our objects in writing it being, not merely to endeavour to ascertain what we consider as truth, for the benefit of our readers, but likewise to incite them to make inquiry and examination for themselves, in order that agencies, of such obvious potency on the human frame, may no longer be permitted free scope if evil, or no longer be debarred from ordinary medical practice, if good.

The internal and external use of water, in the treatment of disease, has been frequently discussed by physicians in all ages, from Hippocrates downwards. Their opinions will be found cited in detail by the systematic writers on the subject of baths, and, among others, by Sir John Floyer and Lanzani. To them we refer such as are sufficiently curious to wish for an exact acquaintance with the subject, in its historical relations. For our present purpose, and to render the history of the medical use of water clear to the less minute student, we will group it under a few convenient heads.

1. According to Lanzani,* the true method of using cold water consists almost entirely in its internal administration, in very large doses, in certain stages of certain fevers. His work is most elaborate in every sense; learned, methodical, and comprehensive. It is divided into two books: the first devoted to an explanation of the causes, symptoms, complications, and nature of fever; the second, showing that copious imbibition of cold water is the best means of combating the symptoms, on scientific grounds, and consequently the best remedy for fever. This is obviously an argument somewhat theoretical, but it is supported by a chapter of

---

* Vero Metodo di servirse dell' Acqua Fredda nelle Febri ed in altri Mali, si interni come esterni. Di Nicolo Lanzani, Medico Napoletano. 2nda edizione. In Napoli, 1723.

cases, and backed by the opinions of a host of learned doctors, the author's predecessors. The actual value of the work is considerably diminished by its scientific character, because many of the doctrines held in its day have now become obsolete, and tend to encumber and obscure, rather than strengthen and enlighten, the practical facts by which they are accompanied. But the same remark applies to the early advocates of other remedies. Lanzani appears to have had no knowledge of the external use of water, nor of its application to the treatment of chronic diseases. He used it in combination with drugs.

Lanzani may be regarded as the representative of a considerable number of writers and practitioners, both in Italy and elsewhere, among whom water has been employed (internally) as the most effectual febrifuge.

2. About the year 1700, Sir John Floyer and Dr. Baynard employed water very freely as an external application, in the ordinary manner of cold bathing, preceding it by a course of physic, and accompanying it generally by copious water-drinking.* Their practice appears to have been chiefly in chronic diseases, such as rheumatism, gout, paralysis, indigestion, general debility, and various nervous affections, in the whole of which a large amount of success is said to have been attained. The baths, at which their cases were treated, were frequently designated by some saint's name. Probably a remnant of superstitious reverence for the saint not only assisted to attract patients to the well, but infused into them a faith in the remedy, which materially promoted their recovery. The practice pursued was simply cold plunging, guarded by certain rules and cautions to prevent accidents.

Sir John Floyer supports his views by the citation of numberless learned authorities, from the Bible to Dr. Mead. He seems to have attached rather an excessive importance to grave precedents, which causes his portion of the conjoint work to savour more of the library than the bedside. At any rate he mingles together practical facts and the opinions of writers, in such intricate relations, that it is not always easy to discover on which he relies most confidently for the maintenance of his tenets. Dr. Baynard, on the other hand, deals more in cases, of which he presents a very abundant collection. His mode of reasoning is particularly pointed and sagacious. No one can leave the perusal of his work without a strong conviction of his being an honest, shrewd, enterprising, and diligent contributor to medical literature.

These writers mention the occasional practice of persons bathing in their shirts, and wearing them throughout the remainder of the day without drying; they also give an instance or two of cases cured or relieved by the application of a wet towel. The former practice is alluded to as an instance of rashness on the part of patients, and the latter is so rarely mentioned, that in neither can they fairly be said to have anticipated Preissnitz in the systematic employment of the wet sheet or wet compress—although both were actually employed by them. They also speak in very favorable terms of a course of cold preceded by a month's warm bathing, but not in the modern hydropathic method of the cold following immediately upon the warm, or upon sweating, which is a practice they carefully deprecate. They seem to have had but a slight acquaintance with the use of cold bathing in fever, or acute diseases, though instances of such practice are given.

---

* Psuchrolousia; or, the History of Cold Bathing, both ancient and modern. By Sir John Floyer, of Lichfield, Knt., and Dr. Edward Baynard, Fellow of the College of Physicians, London. 2d edition. London, 1706.

The following passage from Dr. Baynard, though not strictly a part of our present subject, is a curiosity, and affords a good sample of his peculiar manner. When the period of its publication is considered, it must be regarded, in some of its parts, as a remarkable case of the forestalling of exact experiment by speculative reasoning. Baynard adduces the remarks it contains in support of his hydropathic views; but we need not stay to examine them in that respect. We transcribe portions of the passage:

> "I *conceive life to be an actual flame;* as much flame as any culinary flame is, but fed with its peculiar and proper pabulum, made out of the blood and spirits for that purpose; and my reasons are these, viz.: 1. Life is as extinguishable as any flame is, by excluding the air, &c. For hold your handkerchief close to the mouth and nose of any animal that has lungs, and life is put out, the creature is dead in a moment; there is no skin broke, nor bone broke; no wound, nor bruise; there is your whole man, but dead he is. 2d. No flame will burn without aereal niter, or a *quid aerium*, whatever it be; some will have it a mixed gas of niter and sulphur, but whatever it be, 'tis a *causa sine quâ non*, something without which no flame will burn; and that the lungs serve to this use, and are air-strainers, is very clear to me, by that experiment of the candle and two puppy dogs, put into a great oven, and stopped close up with a glass door to see through; and in a little time, when they had sucked in some, and the candle wasted the rest of the niter, the dogs died, and the candle went out with them at the same instant.
>
> "All ustion, as the *quid inflammabile* wastes, leaves by incineration alkalious and caustical salts, either fixed or volatile, which, from their figure or imbibed fire, become of a pungent corrosive nature, and fix upon the membranes, being nervous and most exquisite of sense and perception, which by irritation cause a light inflammation, which inflammation is called thirst; which salts are melted and washed off by drinking, the grosser by stool with the solid excrements, but those of most solid and subtle particles creep with the chyle into the blood, and have no way out but by the urine. Hence water is the best menstruum to dissolve salts; and that which is most simple and elementary is the best water, as least impregnated; such waters wash off and dissolve their points and angles, by which they prick, sheath, and envelope them in their own pores, and with themselves run off by urine; but if so forced by heat and motion as to disturb them in their passage, the current of urine is checked, and the salts leave their hold of the water, shoot their vortex, and from the channels get into the habit of the body, which, if not dissolved, melted, and thrown off by sweat, they inflame and cause fevers, &c.; nor will they cease their action and inquietude until totally dissolved, or forced back into their common passages, and the salts precipitated and run down by urine. For I look upon the pores and sweat-vents as so many back-doors and sally-ports, by which nature drives out the enemy crept into her garrison. This truth is demonstrated in all fevers, where the caustical salts are not washed off, but remain behind on the glands and membranes, forsaken of their dissolving menstruum, the water, &c., *which that ingenious chemist, Mr. George Moult, by chemical analysis made appear in six quarts of febrile urine, which I sent him, and he found but the thirtieth part of those salts usually found in a sound man's urine*, so that of necessity they must remain behind and be left (like so many French dragoons) to quarter on the blood and spirits at discretion. The history of which is printed in the 'Philosophical Transactions' for some years since.
>
> "Now, that which we call an insensible perspiration is nothing else but the

smoke made from this vital flame, and the pores are the spiramenta through which it passes, and when these are stopped, this smoke is returned and the flame becomes reverberatory, which is sometimes necessary to force an obstruction, for the body has its registers and vent-holes, as well as other furnaces. But to proceed, these salts sometimes crystallize, so that the common menstrua will not touch them, no more than a file will steel or hardened iron, and then it is a true diabetes (and here the physician is at his wit's end, and that no far journey,) then hey! for lime water, quince wine, and other restringents, which, if it were possible, would rather make a coalescence, and tie the knot the harder. No; the cure lies in solution by melting down the salts, which must be done by open, raw, and unimpregnated menstrua, such as the Bristol waters are, as most simple, having least contents in them." (pp. 47 et seq.)

3. About the beginning of the present century, Dr. Currie's practice in fever is well known to have consisted principally of cold affusion, or immersion, in the early stages of the disease, and in certain acute affections of the nervous system. His work is so well known that it is unnecessary to enter into any detail as to its contents.* He seems to have known but little of the application of cold water to the treatment of chronic diseases, as represented by Floyer and Baynard, nor to have employed the copious libations described by Lanzani. He cannot be said to have forestalled Preissnitz in any other respect, than in the prompt and energetic use of cold water in the suppression of acute febrile and nervous affections. He brings a large amount of scientific argument and practical experience to bear out his views. He has also placed in a clear light some points of practice on which important errors previously prevailed, such as the safety of cold applications when the body is heated beyond the natural degree, and the relative value and safety of cold or tepid water, of immersion, affusion, and ablution. On these points his work is of great practical value. We may have occasion to revert to some of them hereafter.

4. The prevalent opinions of medical men in this country, on the general subject of the external use of water, previously to the Preissnitzian era, may be considered to be represented in the article Bathing, in the 'Cyclopædia of Practical Medicine,' published within the last twenty years. The article in question places bathing in a very subordinate position among means available for the actual cure of disease. In its cold form it is recommended as a valuable tonic, used with many restrictions, in nervous debility and other analogous states; and, in its warm form, its use is almost limited to the allaying of irritation in certain disorders, the more formidable symptoms of which are to be encountered by other remedies. Other articles in the same work have done justice to Dr. Currie's views. Beyond this, the medical profession have hitherto done little or nothing with bathing as an instrument of cure. We shall hereafter find reason for believing that a vast superfluity of caution has existed in the employment of this remedy, and that some of the supposed cautions have really increased, instead of diminishing, the danger, as well as destroyed the efficiency of its application.

The author of the article in the Cyclopædia describes cold bathing as partially or absolutely contraindicated in the following conditions: partially, in infancy and old age; pregnancy; indurations, obstructions, or chronic inflammations of internal parts; acute inflammations of the same; chronic inflammations of mucous membrane: absolutely, during menstruation; in great plethora, or tendency to active hemorrhage, or congestions in important viscera; affections of the heart; loaded state of the bowels; great general debility—though then often advantageous after warm water or vapour bath.

---

* Medical Reports of the Effects of Water, cold and warm, as a Remedy in Fever and other Diseases. By James Currie, M.D., F.R.S., Fellow of the Royal College of Physicians, Edinburgh. 2 vols. 2d edition. London, 1805.

5. The ancient Romans were accustomed to produce perspiration by surrounding the person with heated aqueous vapour, and, while freely perspiring, to plunge into cold water. Interesting remains of baths for this purpose, evidently of Roman architecture, and containing fine specimens of mosaic pavement, may be seen in several parts of England; as, for example, on the margin of the Cranham woods, in the village of Whitcombe, about six miles from Cheltenham; at Bignor in Sussex, &c. It is also well known to have been a frequent practice of the Roman youth to plunge into the Tiber, when heated by exercise in the Campus Martius.

The modern Russians, also, as is well known, excite perspiration in a similar manner, and then roll themselves in snow. A somewhat similar practice has prevailed among the North American Indians. The following description of their process was given by the celebrated Quaker, William Penn, to Dr. Baynard.

> "I once saw an instance of it, with divers more in company. For being upon a discovery of the back part of the country, I called upon an Indian of note, whose name was Tenoughan, the captain-general of the clans of Indians of those parts. I found him ill of a fever, his head and limbs much affected with pain, and at the same time his wife preparing a bagnio for him. The bagnio resembled a large oven, into which he crept, by a door on the one side, while she put several red-hot stones in at a small door on the other side thereof, and then fastened the doors as closely from the air as she could. Now, while he was sweating in this bagnio, his wife (for they disdain no service) was, with an axe, cutting her husband a passage into the river (being the winter of '83, the great frost, and the ice very thick), in order to the immersing himself, after he should come out of his bath. In less than half an hour he was in so great a sweat that, when he came out, he was as wet as if he had come out of a river, and the reek or steam of his body so thick, that it was hard to discern any body's face that stood near him. In this condition, stark naked, he ran into the river, which was about twenty paces, and ducked himself twice or thrice therein, and so returned (passing only through his bagnio to mitigate the immediate stroke of the cold) to his own house, perhaps twenty paces further, and wrapping himself in his woollen mantle, lay down at length near a long (but gentle) fire, in the middle of his wigwam, or house, turning himself several times, till he was dry, and then he rose and fell to getting us our dinner, seeming to be as easy, and well in health, as at any other time."—(Baynard, pp. 103-4.)

The extraordinary revivifying effect of the cold plunge bath, after the system had been excited by artificial heat, is testified by various evidence of the most unquestionable kind. Numerous travellers have spoken of this very enthusiastically; among others, Stevens, the American (in his 'Incidents of Travel'), who took a Russian bath, after a most fatiguing journey, and came out of it, he says, quite a new man. We have had similar information from more than one private source. This practice, however, similar as it is to that of Preissnitz, has never, until his time, been extensively, if at all, employed in Europe as a means of curing disease.

6. In the foregoing synopsis are contained the principal forms in which cold bathing and water drinking have been used in the treatment of disease, before these means were so vigorously adopted by Preissnitz. It will be obvious that, from none of the writers mentioned, could he have learned his bold and comprehensive practice. In his method are combined those of Lanzani, Floyer, and Currie, accompanied by novel and powerful processes, to which those writers were entire strangers. The douche, the wet sheet, the sweating blanket, the cold plunging after sweating, the wet compress, the sitting bath (sitz-bath), must be allowed to be, in a great measure, peculiar to the Graeffenberg peasant and his disciples. From the same source have proceeded some important precepts on the subject of diet and regimen. Preissnitz, moreover, is distinguished from all the authorities quoted, by his entire abandonment of drugs.

Vincent Preissnitz was originally a small farmer, residing at Graeffenberg, near the town of Freiwaldeau, in Silesia. He is about fifty years of age. The following is a description of him by Sir Charles Scudamore:

> "Of Preissnitz himself I shall say a few words, and describe my impressions on first seeing him. His countenance is full of self-possession; rather agreeable; mild, but firm in expression; with an eye of sense, and a pleasing smile. The smallpox, and the loss of some front teeth from an accident, impair his good looks. His manners are sufficiently well-bred. On closer acquaintance, you discover that he is quick in perception; is reflective; prompt, however, in decision; simple and clear. He inspires his patients with the most entire confidence, and he exacts implicit obedience." (A Medical Visit to Graeffenberg, pp. 2-3.)

Other travellers give a similar description of Preissnitz. They all agree in stating that he is a most arbitrary and tyrannical despot, issuing laws as irrevocable as those of the Medes and Persians, commanding obedience with a haughtiness that might well excite admiration and envy even in an autocrat, and exciting as much fear in his patients as is found in the subjects of the Grand Turk himself. He is also represented as remarkably cool and collected in emergencies, ever ready with his remedy on occasions of danger, and possessed of an imperturbable self-reliance. These traits suffice to prove that he is a man of original and powerful mind, exactly adapted to carry out a novel and startling practice. While his firm and decided manner is calculated to secure the confidence of his patients, his coolness and self-reliance enable him easily to bear the responsibility by which such confidence is attended.

His practice originated in a succession of trifling accidents, by which he was led to employ bathing in a neighbouring spring for the relief of disease. It is not necessary to give them in detail. Success in these first attempts procured him a local renown, and he became the village doctor. From villagers his fame soon spread to patients of a higher rank, and Graeffenberg gradually became the resort of the hipped and the halt from all the surrounding district. By these his praises were sung louder and louder, until all the world began to furnish him patients by the hundred. He now possesses an enormous establishment, capable of containing several hundreds of patients, which is almost constantly crowded with ladies and gentlemen of every degree, and from every nation; while his disciples and followers, as is well known, have spread themselves throughout the world, and maintain, in every country, numerous and flourishing establishments formed on the original model of Graeffenberg.

His treatment, although apparently constructed of such simple elements, is capable of being varied almost *ad infinitum*, according to the peculiarities of the case or the fancy of the prescriber, and of being rendered so powerful, as often to excite in the patients and spectators apprehensions of danger—and sometimes, no doubt, to produce it in reality. It is scarcely too much to say that he has modified the application of water, and some very few other means, in a manner so ingenious as to render them no imperfect *nominal* substitute, at least, for most of the drugs in the pharmacopœia. He has his stimulant, his sedative, his tonic, his reducing agent, his purgative, his astringent, his diuretic, his styptic, his febrifuge, his diaphoretic, his alterative, his counter-irritant. Combined with these are peculiar regulations as to diet, dress, and regimen. The following is his general mode of proceeding.

In his first interview with the patient, after hearing sufficient to give him a rude insight into the locality and general features of the malady, Preissnitz proceeds to investigate its suitableness to his method of cure. He does this by sprinkling the surface of the body with cold water, or witnessing the taking of a cold bath, and then watching the development of reaction. If this appears in a certain amount of activity, he pronounces the case appropriate for his treatment; if not, he advises the abandonment of all hydropathic intentions. This is a mode of ascertaining the power of the constitution quite original, and it cannot be said to be unscientific. The power of resisting the external application of cold is a most essential conservative property of the animal system, and the degree to which it exists must be regarded as, in some respects, a criterion of the amount of *vis medicatrix* possessed by the patient. We see no very decisive reason for pronouncing it a more fallacious guide than the orthodox custom of feeling the pulse. The only objection to it that occurs to us is that it may not be always free from hazard.

This point being satisfactorily determined, the patient is straightway admitted into the mysteries of the cure. In the first place, he finds himself restricted to a peculiar diet. Every stimulant is absolutely prohibited, from brandy and claret, to mustard and pepper; so, also, are most of the luxuries imported from foreign parts, such as tea, coffee, and every kind of spice. The meals consist of three—breakfast at eight, dinner at one, and supper at seven or eight o'clock. For breakfast, cold milk is the beverage, and bread and butter its only substantial companions. At dinner, there is no other restriction than those above named. Supper is a repetition of breakfast, with the occasional addition of preserved fruit or potatoes. Throughout the day, no warm beverage whatever is permitted, and much of the food is brought to table considerably cooled. As some compensation for these manifold deprivations, the patient is allowed to gratify his appetite with every reasonable variety, and a free abundance, of substantial and nutritious food. He finds it a maxim that generous diet will promote his recovery, the treatment being responsible for preventing surfeit. He no longer finds an embargo laid on fruit and vegetables, and is not expected to dine seven days in the week off dry bread and mutton chops. So that, on the whole, there is perhaps about an equal amount of indulgence and restriction, as respects diet, to a patient coming to the Graeffenberg rules from those of some fashionable physician in London.

In the next place, the majority of patients are directed to enter upon a course of water-drinking, the quantity of water varying from five or six to thirty or forty tumblers in the twenty-four hours. A large portion of this is taken before breakfast, the rest at suitable periods after meals, so as not to interfere with digestion, with frequently a glass or two the last thing at night. Exercise is generally advised at the time of water-drinking, except when this accompanies some other process of treatment incompatible with it.

A third rule insisted on is, that every patient shall take a large amount of exercise during the day. This is, to some degree, indispensable after the cold baths, as a means of procuring the necessary reaction. Walking in the open air is the mode generally selected, when possible. In case of bad weather, or lameness, other plans are contrived; such as gymnastics, sawing or chopping wood, &c. As a general rule, every patient is required to take a long walk before breakfast. It is a *vexata quæstio*, we believe, among hydropathists, as among doctors, whether the patients should rest or walk immediately after a meal; but the water-doctors generally incline to advise very gentle exercise at such times; and, we believe, properly. The well-known experiments on greyhounds, and such other convincing facts, are counterbalanced, to say the least, by the habits of the working man, who proceeds to his labour as soon as he has swallowed his dinner, and rarely suffers from so doing.

After these preliminaries, and the case being pronounced suitable for the treatment, the next morning witnesses the patient's initiation into more active proceedings. At any early hour of the morning, varying according to the time required for the operation about to be undergone, a bath attendant enters with the formidable machinery for the administration of a rubbing with a wet sheet, a packing in the dry blanket, or a packing in the wet sheet. The first of these processes consists of throwing a wet sheet over the whole person, and applying upon it active friction of a few minutes' duration. A glow is thus excited. The patient then dresses, takes his water, and sets forth upon his morning's walk. The second of the above three operations requires the patient to be enveloped in several blankets, with perhaps the superincumbence of a large feather pillow, until free perspiration is excited, which generally requires a period of about three hours. When the perspiration has continued the prescribed time (from fifteen minutes to an hour, or more), the patient is subjected to some kind of cold bath, either by the wet sheet, as just described, by pouring water over the person from pails or watering-pots, or by taking a plunge bath. This being followed by friction and water-drinking, the morning's proceedings are concluded by exercise. Packing in the wet sheet is similar to the foregoing, with the addition, next the skin, of a sheet wrung out of cold water. It is generally of shorter duration, as forty-five minutes or an hour, the object being to excite a glow, instead of perspiration. It is followed by cold bathing, as just described. During the packing, in both instances, some glasses of cold water are imbibed through a tube.

At other parts of the day, other portions of the treatment are applied, such as the sitz-bath, the douche, the shower-bath, head-bath, foot-bath, &c.

The sitz-bath is a tub of cold water, in which the patient sits for a period varying from a few minutes to an hour, or even longer, using constant friction to the abdominal region. The other baths mentioned in this paragraph need no description. These, as well as the former processes, are sometimes repeated during the day. In certain cases the day's proceedings commence with some of them, in place of those previously mentioned. A rubbing with the wet sheet is frequently employed before getting into bed at night.

In fever, from whatever source, the patient is enveloped in a succession of wet sheets, renewed as often as they become warm, for a period varying with the intensity of the case—say, from 30 minutes to 5 or 6 hours. In other similar cases, cold immersion or affusion is employed with the same view—viz. to reduce the morbid heat of the system.

The *umschlage*, or compress, is an essential and seldom-omitted part of the treatment. It is a cloth, well wetted with cold water, applied to the surface nearest to the supposed seat of the disease, securely covered with a dry cloth, and changed as often as it becomes dry during the day. It is sometimes covered with a layer of oiled silk, which, by impeding evaporation, prevents the inconvenience of frequent change. This compress speedily becomes warm, and remains so until dry. It is termed a heating or stimulating bandage. In cases of superficial inflammation it is more frequently changed, so as to be kept cold, whereby its effect is just the reverse, being then a local antiphlogistic.

In some establishments the sweating has been effected by other means, than the simple envelopments of Preissnitz, as by the vapour-bath, or a chamber highly heated by a stove. We have heard of a temperature of 180°, and even that of 198° Fahrenheit, being employed for this purpose. The blankets used by Preissnitz are very bad conductors of caloric, therefore they cause the heat given off by the body to be accumulated around its surface, by the lengthened influence of which the sudorific action is effected. This process differs in no other manner than in degree and rapidity of effect from exposing the same surface to heat of any other origin. The animal heat, when once evolved, becomes a quality of the surrounding atmosphere. Being kept in contact with the body by blankets, it constitutes an artificial elevation of temperature, and nothing more. Therefore, in cases where active sweating is required, we can suppose no disadvantage to result from using other kinds of artificial heat, and can easily imagine advantages in a higher temperature than that attainable from animal heat alone. But we would limit this remark to dry heat. Aqueous vapour, by a well-known law, impedes evaporation, and would therefore restrict the full completion of the sudorific process. For this reason it is used to prevent plants from parting with their moisture in hot-houses. For the same reason it should *not* be used when the intention is to promote the removal of moisture, or to promote perspiration.

A point uniformly insisted on by Preissnitz is, that his patients should abstain from wearing flannel next the skin. When we consider how generally the use of this article of clothing has been advised by physicians and adopted by invalids, especially in this country, we can easily conceive that strong prejudices will exist in the minds of patients against relinquishing it. Yet it appears to be almost universally discarded by hydropathists, and, as far as we have learnt, without any mischievous consequences.

Another maxim of Preissnitz is, that his patients are never to take any kind of drug. It should be remarked that, not being licensed to practice medicine, it would be illegal for him to administer drugs. So that it does not follow, from his disuse of them, that he himself would be opposed to their use *in all cases*, much less that their use is in any way *inconsistent* with his practice. His medical disciples, not being similarly restricted, so far as we can learn, usually employ drugs occasionally, though sparingly.

How are we now to proceed, in order to arrive at a just appreciation of the value of the means thus briefly enumerated? The more usual course would be to enter upon an examination of the practical results, as published by hydropathic writers. But, in the present inquiry, this plan would scarcely answer; for the means employed are so strange, so much at variance with those by which disease is commonly treated, and not a few of the reporters are so little entitled to claim credit for even a capacity to report medical results truly, that the greater part of our readers would disbelieve the alleged facts, rather than admit the principles they would carry with them. It will be more proper, therefore, to omit matters of evidence for the present, and to see if we can find in hydropathic practice any conformity with the principles on which we should estimate the merits of any other new remedy.

If a new vegetable were imported, or a previously unknown chemical substance discovered, and we were called upon to use it as a medicine, we should first inquire whether it possessed any of those qualities which are regarded as constituting medicinal virtues. We might assume that we are sufficiently acquainted with the characters of most diseases, to pronounce what description of influence would have a counteracting effect upon them. It would then remain to inquire, whether the qualities possessed by the article in question were of a kind to lead us to expect any description of such influence from their operation. If they were not, we should be indisposed to try the remedy until well assured, from abundant and unquestionable practical evidence, of its curative powers. If they were, we should be inclined to give it a trial, even if the proofs of its remedial properties were not unexceptionable. For instance, if the article under consideration merely possessed a nauseous taste, a specific colour, or a powerful odour, it would offer little inducement for an experiment of its medical powers, because those qualities are not known to possess any intrinsic influence over any diseased condition. But, if it were a purgative or a sedative, no one could hesitate to recognize it as *à priori* entitled to a trial by physicians; because experience has taught us that, by the means of purging or tranquillizing, certain diseases or morbid symptoms may be cured or relieved. And, since it is the case with many of our present remedies, that with the property we wish to employ is combined another we would gladly avoid (purgatives being debilitating, sedatives narcotic, &c.), and with their amount of usefulness is thus associated a certain tendency to mischief,—if the new remedy presented to us appeared to possess the essential quality, and to want the mischievous power of that otherwise used for the same purpose, we should be still more desirous of availing ourselves of it in practice.

If we apply these remarks to Hydropathy, as practised by Preissnitz, the first inquiry ought to be, does it furnish the physician with instruments which he, as a skilful workman, can undertake to employ? Does it contain, among its various machinery, any really therapeutic means, any powers capable of carrying out the indications which we regard as palpable in many diseases? Can it evacuate, can it brace, can it tranquillise? We cannot entertain the idea that the professors of Hydropathy have hit upon any grand secret concerning the origin or nature of diseases, or the philosophy of their removal. Such a supposition, were it a necessary article of faith in the hydropathic creed, would render us the most obstinate of sceptics. But, if the practitioners of this new school profess merely to have introduced more efficient, or less dangerous, means of fulfilling the purposes which all physicians have in view in treating disease, we are willing to give them a patient and impartial hearing. Or, if they profess nothing of the kind, and reject such an idea with contempt,—if, nevertheless, their system appear *to us* of the nature we are indicating, we can still entertain it with the hope of discovering something of good in it.

Let us now inquire, then, on physiological and pathological grounds, supported by some personal experience, what appear to be the effects, or among the effects, of a course of water treatment according to the Preissnitzian system.

1. In the first place, we remark the careful withdrawal of all stimulants from internal parts. In this hydropathy is at once distinguished from ordinary practice. The refinement of civilized life, and the complicated affairs of society, prevent the human frame from being treated entirely as a machine. The body is compelled to undergo an usage not always suitable to its welfare, in consequence of its having to minister to the mind. The exhaustion of the latter, from exertion and excitement, is restored by artificial stimuli applied to the former. These are generally directed to parts ill adapted for their reception. Thus, the stomach, constructed to digest simple food, and to admit fluid at the impulse of thirst, becomes the vehicle of conveying to the nervous system alcohol in its various forms, and other similar fluids. These are unnatural to the stomach itself, though grateful to the nerves. Consequently, the mucous lining of the alimentary canal may suffer in the attainment of an object required only by the nervous system. This is, possibly, the very origin of a proportion of those manifold chronic ailments known under the terms dyspepsia, hypochondriasis, bilious affections, &c., and is unquestionably an aggravating cause in many. To the treatment of these affections the physician brings his purgatives, his carminatives, his anodynes, his stomachics. But it is to the surface of the same unfortunate membrane that they are all applied; and it frequently results, that, when they relieve temporary suffering, they often leave the general health worse than they found it. From this predicament hydropathy professes to be entirely exempt, by abstaining from artificial interference with internal mucous membranes.

2. In the next place, the Hydropathists adopt a system of diet, such as other practitioners seldom venture to prescribe. If a person, suffering from constipation, or any of its long train of attendant ills, applies to an ordinary physician, he is probably told scrupulously to avoid fruit, pastry, and all vegetables, except, perhaps, a favorite one, or, it may be, two. He is also cautioned against the use of veal, pork, even beef, and new bread. We have known such a patient ordered to live for months—we might say, years—constantly on mutton, and bread never less than five days old. This case is neither singular nor infrequent. What is the consequence of this? The patient is compelled to take aperient pills and draughts every day, or every other day; to stimulate the digestive organs (rendered torpid by the use of so monotonous a regimen) by occasional glasses of sherry or porter; and, to compensate the deficient nutrition obtained from so barren a source, by indulgence in strong tea and coffee. Such a patient goes to a hydropathic establishment, and is straightway ushered into a *salle-à-manger*, in which he finds all the variety of food customary at a foreign *table-d'hôte* dinner, and is told to obey the dictates of his appetite. He does so, timidly at first, and apprehensive of direful consequences, but he finds, to his astonishment, that he can take the forbidden luxuries of broccoli, turnips, veal, game, puddings, and fruit, with as much impunity as the never-varied mutton and dry bread, to which he was previously restricted. This is an occurrence so frequently experienced, and so universally attested by hydropathists and their patients, that we cannot refuse to admit it as a point attained by *their system*—therein being comprehended the water *and all its accessories and concomitants.*

3. A third important principle of hydropathic treatment is, that almos all its measures are applied to the surface. It is one of the most formidable difficulties with which the ordinary physician has to contend, that nearly all his remedies reach the point to which they are directed, through one channel. If the brain requires to be placed under the influence of a sedative or a stimulant, if the muscular system demands invigorating by tonics, if the functions of organic life need correction by alteratives, the physician has no means of attaining his object, except by inundating the stomach and bowels with foreign, and frequently to them pernicious, substances. In being thus made the medical doorway to all parts of the system, and so compelled to admit every description of therapeutical applicant, the organ of digestion is contorted to a purpose for which it was never intended. The consequence is, that it has to be consulted before we enter upon the treatment of any case, and it often forbids our availing ourselves of remedies, or plans of action, which are plainly, perhaps urgently, indicated by the condition of other organs, or of the system at large. Thus, to take the three cases above mentioned: how often do we find that one stomach will neither bear ether nor opium; another is injured by steel; and others are intolerant of mercury. The two latter remedies are peculiarly illustrative of these remarks. Iron is employed to raise the tone of the general system, but it occasions constipation by its action on the alimentary canal; therefore, in order to counteract this portion of its effect, it can only be used in conjunction with aloes, or some other purgative, the tendency of which, as respects the system at

large, may be exactly the reverse of that of the steel. With mercury the case is just the opposite. We wish to introduce it into the system, but it is purgative as well as alterative and antiphlogistic, and the former quality often renders very difficult our attaining the benefit of the two latter. The physician, then, is frequently placed in the dilemma, either to injure the stomach in an attempt to relieve other parts, or to leave the latter to their fate, because they can only be rescued at the peril of the former. His only mode of escape from this predicament is, to employ a legion of *adjuvantia*, *dirigentia*, and *corrigentia*, in the multiplicity and confusion of which it is by no means easy to make out so clear a balance of power as shall enable him clearly to foresee which kind of action, in the *mêlée*, will get the uppermost; and, unless he be well skilled in chemistry, he may unconsciously prescribe a dose so scrupulously guarded as to be neutralized and altogether impotent.

Of course we do not conclude that hydropathy has discovered a remedy for this difficulty; but its own plan of proceeding is not similarly embarrassed, because it deals with outward instead of inward parts. Whether it can produce an efficient substitute for steel, mercury, opium, and other remedies, to which we are alluding, is altogether another question, and one which its professors must bestir themselves to solve, by the careful record and honest publication of their successful and unsuccessful cases.

4. Fourthly, Hydropathy employs a system of most energetic general and local counter-irritation. It has been held by some medical philosophers, that two kinds of morbid action cannot co-exist in the same individual. According to this theory, if we can set up an artificial, but harmless, disease by treatment, its development will be attended by the departure of any other disorder that previously existed. Thus is supposed to be explained the operation of mercury in curing various diseases, the disorder arising from its own action being easily disposed of afterwards. We attach no value to this dogma as a dogma, but it serves to embody a large number of well-known facts, and may be as properly appropriated by hydropathists as by other practitioners. By the diligent employment of hydropathic machinery, due regard being had to the constitutional vigour, a condition is often excited, termed by hydropathists *the crisis*. This sometimes consists in the appearance of various cutaneous eruptions; sometimes it is characterized by a series of boils, more or less severe; in other cases its leading feature is disturbance of the function of some internal organ, creating diarrhea, abnormal urinary discharges, vomiting, &c. In general this effect is trifling, and seldom proceeds to such a degree as to excite alarm, or to give cause for special interference; so that the measures which have led to its appearance are in most cases continued, and in some even increased, until it has run through its course and subsided. This is not always the case: sometimes it proceeds to a more serious length, and requires careful management to prevent mischief; the boils, in particular, are frequently very troublesome;—even death has, in a certain proportion of instances, ensued, either as an immediate or remote consequence of the so-called crisis.

Whatever the crisis may be—or whether what is so called *be* a crisis in reality—there is no disputing that it results from the operation of a powerful system of counter-irritation—or of irritation at least. It is to this that we now wish to direct attention, because we suspect that in it is contained the true explanation of the good effects of the water-cure in many chronic cases.

5. A fifth physiological feature of the water-cure is the number of *coolings* to which the body is subjected during the day. The generation of caloric in the animal system has been traced to its real source. It results from the *burning up* of waste matter, which, by accumulation, would become injurious. The oxygen of the atmosphere, admitted into the lungs by inspiration, traverses the various blood-vessels of the body, and, in the minute capillaries, unites with carbonized substances. The union produces the carbonic acid emitted from the lungs in expiration, and is attended with the development of what is called animal heat. It is obvious that lowering the temperature of the body, within certain limits, by awakening an uncomfortable sensation in the nerves, would induce increased activity in this calorific process, in order to maintain or restore the average degree of warmth. This increased activity could only be supported by an additional consumption of carbonized matter. If the carbonized matter were already there, and if its existence constituted the disease, or an important part of it, as is probably sometimes the case, a perfect cure would result from its removal. But supposing there is no such matter present, what then would be the consequence of stimulating this decarbonizing operation? The consequence would certainly be, that the constituents of the tissues themselves would be consumed, in order to supply the pabulum required by the oxygen. This would as certainly excite an effort at restoration, by which the digestive organs would aim to renew to the tissues the amount abstracted by the oxygen. In other words, the appetite would be increased.

Hence it is that more food is required in cold climates than in warm—in winter than in summer. The greater consumption necessary to maintain equal temperature in cold weather, can only be met by increased supply. What, in a vague and general manner, arises from the ordinary progress of the seasons, may be rendered methodical and profitable, by the careful interference of art.

It has been urged that the effect here considered would equally result from exposure to cold air, as to cold water. In the words of Mr. Herbert Mayo, "This is not only entertaining, but satisfactory as far as it goes; and admits very well of being popularly and loosely brought forward in favour of cold bathing: but unluckily it is as much or more in favour of our living in Nova Zembla, as of our resorting to Graeffenberg." (p. 5.)

The same intelligent writer proceeds to notice other modes of exposure to cold, which are found to produce evil instead of good, which are, indeed, familiar as the frequent causes of serious disease, and against which we are of old cautioned:

"Nudus ara, sere nudus,—habebis frigora, febrim."

It is singular enough that this very argument, now employed to discountenance the use of cold bathing, is the very strongest theoretical argument in its favour, as was long ago pointed out by that very sarcastic writer, Dr. Baynard, in the following anecdote:

"Here a demi-brained doctor, of more note than nous, asked, in the amazed agony of his half-understanding, how 'twas possible that an external application should affect the bowels and cure the pain within. Why, doctor, quoth an old woman standing by, by the same reason that, being wet-shod, or catching cold from without, should give you the gripes and pain within." (p. 119.)

If a rude exposure of the surface to cold and wet is capable of producing internal disease, there is no doubt that a close relation exists between those agents and the morbid conditions of internal parts. Therefore, if they could, by skilful management, be so applied as to excite an opposite effect from that to which their bad consequences are due, they would then become equally powerful means of removing disease. This is the very thing that Preissnitz and his disciples profess to have done—and to do.

Let us, then, consider a little further the consequence of repeated applications of cold, supposing, for the sake of argument, it is used with due reference to the constitutional powers, so as to create an increased activity of the vital functions. It appears to us that this is exactly the thing needed in the treatment of a great many cases of chronic ailments. It is easy enough to construct methodical catalogues of organic lesions and their symptoms, and to assign, on paper, a "local habitation and a name" for every malady that is to require our treatment. But the truth is that, practically speaking, there are a vast number of cases in which the symptoms may be said to constitute the only disease that can be detected, and in which they point rather to a general torpidity or derangement of all, or almost all, the vital functions, than to special change or disturbance in any particular organ. Many cases known as indigestion, gout, rheumatism, liver complaints, or nervous affections, come under this description. In a large portion of such cases, and their like, we could conceive the practice of Preissnitz to be peculiarly beneficial, if it consisted in nothing more than the frequent application, and skilful adaptation, of cold water. It was mainly by this means that the cures described by Floyer and Baynard were effected, simple cold bathing having been almost their only instrument.

6. Another physiological feature of hydropathic treatment consists in its creating a large amount of stimulation in the system. This stimulation is of a peculiar kind, and very different from that produced by alcoholic fluids or pharmaceutical stimulants. The difference is in its not awakening abnormal activity, to be succeeded by abnormal depression, in the nerves and organs of circulation, as is done by the stimulants just mentioned. The fall of a heavy douche, the sudden plunging into a cold-bath with speedy exit, active friction in a shallow bath, are means of stimulating the system in the manner here intended. The effect, we are told, is manifested in the altered look of the patient after taking the bath, in his freshened cheek, his brightened eye, his elastic step, his cheerful tone. But it is *not* manifested in a quickened pulse, or a heated imagination, nor followed by exhausted energy or lowered spirits. This is the description given by hydropathists (whose practice we are not teaching but describing) —and which we have ourselves heard given by patients. It is also said that drinking, in rapid succession, several glasses of perfectly cold water has a decidedly stimulating influence on the system. If these descriptions be correct of hydropathic stimulants, that they are powerful as well as innocuous, exciting and not exhausting, they constitute a valuable

instrument in the treatment of disease, and deserve the more careful attention of physicians.

We happen to have been acquainted with a case of a lady who was at a hydropathic institution for the treatment of very aggravated chronic rheumatism. Her general powers were much shaken, and she had been unable to walk at all for a period of about four years, before undergoing this system of treatment. After several weeks of sweating and cold plunging, locomotion began gradually to return. The first indication of this was, that she could walk a few steps *immediately after leaving the cold bath.* For a considerable time this continued to be the only occasion of her being able to walk during the day, though she afterwards made considerably further progress. We mention this case because we can guarantee its truth, and it always appeared to us a striking and instructive instance of the stimulating property of a cold bath.

7. A still more important and less questionable quality of the water cure is its power of lowering the system to any extent, without any of the debilitating means otherwise used for that purpose. In a general inflammatory or febrile condition of the body, a lengthened immersion in cold water, or envelopment in a succession of cold wet sheets, would reduce the temperature and force of circulation to the most extreme degree. These means are, to the functions of life, what an extinguisher is to a flame. Their reducing power can be gradually applied up to the point of actual extinction. Anywhere short of that, withdraw the means, and the flame, whether of oil or of life, gradually resumes its previous brilliancy.

In the treatment of febrile diseases an important indication is to reduce the morbidly increased activity of some of the organic functions, most distinctly manifested in the circulation and the temperature. For this purpose the great instrument heretofore most in use is blood-letting, as being our only certain and expeditious method of reducing the frequency, force, or fulness of the pulse. So that, in order to suppress febrile action, we hazarded occasioning a more or less lingering debility. The *post hoc*, whether *propter hoc* or not, is too frequently a protracted convalescence, during which the patient is in constant danger of relapse. The mortality that occurs during convalescence after fever, from recurrence of the original disease, from some of its numerous sequelæ, or from the accidental inroad of some other disorder, is so considerable as to render this a period of great anxiety to the patient and the physician. It is a question deserving of cautious and dispassionate investigation, whether any portion of the liability to these mishaps is attributable to the bleeding, purging, salivating, and low diet, employed in removing the fever.

In some of the cases of fever described by Currie we cannot fail to be struck by the rapidity and completeness of the cures effected by cold affusion or immersion, when used sufficiently early. The disease appears to have been suddenly checked or destroyed. In the course of a few hours, or a day or two, a patient threatened with, or labouring under, a dangerous fever, was restored to perfect health. No period of debility ensued, no organs were found to have been seriously or permanently injured. The result of his well-known treatment, by cold bathing, of the fever which appeared in the 30th regiment is thus described:

> "These means were successful in arresting the epidemic; after the 13th of June no person was attacked by it. It extended to fifty-eight persons in all, of which thirty-two went through the regular course of the fever, *and in twenty-six the disease seemed to be cut short by the cold affusion.* Of the thirty-two already mentioned two died. Both of these were men whose constitutions were weakened by the climate of the West Indies; *both of them had been bled in the early stages of the fever ;* and one of them being in the twelfth, the other in the fourteenth day of the disease, when I first visited them, neither of them was subjected to the cold affusion." (Vol. i. p. 13.)

Again:

> "In cases in which the affusion was not employed till the third day of the fever, I have seen several instances of the same complete solution of the disease. I have even seen this take place when the remedy had been deferred till the fourth day ; but this is not common." (Ibid. p. 23.)

In contemplating these facts, we are driven seriously to ask, not only is not the debility consecutive to fever partly occasioned by the remedies employed in its treatment, but are not its attendant local and organic lesions in great measure produced by the febrile paroxysm itself? And could they not be avoided, by boldly applying a remedy by which this febrile condition would be more speedily subdued? The real nature of fever is, unfortunately, beyond the reach of our present knowledge. We only recognize the disease in its causes, its symptoms, its complications. In them we perceive much to lead us to answer the above questions in the affirmative. It is peculiarly a general disease. Its local characters usually appear subsequently to its general development, and wear much more the aspect of consequences than of causes. Almost any of the local complications of synochus, or typhus, may appear in exanthematous fevers where they cannot be causes.

It appears to us to be a most important subject of inquiry whether a very serious fallacy does not pervade the medical profession at present as to the best manner of employing cold water in fever. Dr. Currie says:

> "When the affusion of water, cold or tepid, is not employed in fever, benefit may be derived, as has already been mentioned, though in an inferior degree, by sponging or wetting the body with cold or warm vinegar or water. This application is, however, to be regulated, like the others, by the actual state of the patient's heat and of his sensations. *According to my experience, it is not only less effectual, but in many cases less safe ; for the system will often bear a sudden, a general, and a stimulating application of cold, when it shrinks from its slow and successive application.*" (Vol. i, p. 73.)
>
> "It is evident De Haen was not regulated, in his use of external ablution with cold water, by rules similar to those which I have ventured to lay down from several years' experience. Instead of pouring water over the naked body, *he applied sponges soaked in cold water to every part of the surface in succession for some time together, in my judgment the least efficacious, as well as the most hazardous, manner of using the remedy.*" (Ibid. p. 84, note.)

This is a remark which we suspect to be of very great importance, and to contain the real secret of much of the difference, as to the treatment of fever, between hydropathists and the regular faculty. Modern physicians have professed to regard Dr. Currie as a very high authority on this point, and his work is constantly quoted as the most enlightened guide for the use of water in fever ; but the above opinion and precept have been, of late years, entirely disregarded, and the converse has been made the rule of practice. In the article on Bathing, in the Cyclopædia, formerly referred to, the author says:

> "The only cases in which refrigeration is required as a remedy are those in which the animal temperature is elevated above the natural standard; and this happens only *in febrile diseases.* To ensure refrigeration, the water should be applied *at first only a little below the temperature of the skin*, its heat being insensibly and gradually reduced, *but never below that of tepid, or, at most cool.* The *gentlest mode of applying it is the best, as with a soft sponge*; and the process should be persevered in, without interruption, until the desired effect is produced." (Art. Bathing, Cyclopædia of Practical Medicine.)

We believe this mode of applying water in the treatment of febrile diseases to be that which has for many years generally prevailed, not from ignorance of the precepts and practice of Dr. Currie, but from a general belief that fever once formed could not be extinguished by the cold affusion as recommended by him. The hydropathists have renewed his system in its full boldness. It is, therefore, a question of the first interest, on which side does reason preponderate?

On carefully examining the cases of fever reported at length by Currie and Lanzani, it will be seen that their cures were effected by what may be termed a process of reaction. The immediate consequence, in most cases, of the copious libation "*ultra satietatem*" of the one, and the affusion, or immersion, of the other, were perspiration and sleep. These constituted the reaction. When exacerbation of the fever ensued, and required a repetition of the remedy, it occurred several hours after the cold application, when the period of reaction had long passed over; and evidently proceeded, not from the consequences of the cold treatment, but from the non-removal of the diseased action. The cold appears to have acted in a most decidedly *medical* manner, with a palpable and immediate succession of consequences altogether different from what the gradual coolness of the sponge and tepid water can be expected to produce. If these cases are correctly stated, as they appear to be, it is preposterous to confound the febrile paroxysm with reaction from a cold bath, or to expect any portion of the beneficial effect of cold immersion in fever, from tepid or cool sponging. The two kinds of treatment are in no measure similar.

But it may be supposed there is a danger in the sudden and active employment of the cold bath in fever. We suspect that this is entirely imaginary. Dr. Currie was certainly very bold in its administration, and had extensive experience of its effects. In the second edition of his work he says:

> "I have thus related all the instances which have occurred to me since the last edition of this volume (a period of five years of extensive and attentive observation), in which the affusion of water on the surface of the body, cold or tepid, proved either less beneficial in its effects in fever than I had formerly represented it, or entirely unsuccessful. I would add, if any such had occurred, the instances in which this remedy had appeared to be injurious. But experience has suggested to me no instance of the kind, and extensive as my employment of the affusion has been, *I have never heard that it has suggested*, even to the fears or prejudices of others, *a single occasion of imputing injury to the remedy.*" (Vol. ii, p. 25.)

This statement, which does not appear to have been assailed, goes far towards proving the innocence, as his numerous cases do the curative powers, of reaction in the treatment of fever. We certainly cannot quarrel with hydropathists for seeking to revive, in its real character, a method supported by so high an authority.

8. It is scarcely necessary to remark that a judicious system of cold bathing is a valuable tonic. This has been always known; but it has not been so widely recognized in practice as in doctrine. It has been thought necessary that cases for cold bathing should be carefully selected; that they should consist only of such patients as have unimpaired constitutions; that certain diseases were absolute contraindications against the use of this remedy; that it is a treatment requiring unquestionable vigour in the patient, and skill in the physician to employ it without injury. It is scarcely too much to say it has been regarded as a treatment rather for the strong than the weak, and as tending rather to reduce than augment the powers of the system—and yet it is called a tonic. This is an illogical paradox not quite solitary in medical literature. The cold bath seems to be professionally employed to strengthen the body, as temptation is to strengthen virtue, by furnishing an enemy to struggle against. Thus it is considered more as a test than as a source of strength.

The hydropathists have discarded this excessive precaution, and boldly used their remedy as a tonic, wherever a tonic is required. They have administered it to the young and the old, the weak, the bilious, the gouty, the scrofulous, the dyspeptic, and the paralytic. Neither mucous membranes nor mesenteric glands, infantile weakness, nor senile decrepitude have stood in their way. To almost all cases, all ages, and all constitutions, their method has been applied. Unless it can be shown that this all but universal administration of the system has produced serious evils, we are actually driven to admit that it is in the same proportion safe. And we are bound to admit—though we have known some instances where the practice has been seriously injurious, and have heard of others of a similar kind—that the proportion of bad consequences has not appeared to us greater than in the ordinary modes of treating similar diseases. The practice of the hydropathists is so open, and their disciples so numerous, that the innocence of their proceedings may be said to be established by the absence of evidence to the contrary. We cannot enter any circle of society without encountering some follower of this method, ready to narrate a series of *psuchrolousian* miracles, prepared to defend, and zealous to applaud the Preissnitzian practice; but few or none come forward with satisfactory evidence of anything like general mischief having resulted from its general practice. Judgment must, therefore, be entered by default against its opponents, and hydropathy is entitled to the verdict of harmlessness, since cause has never been shown to the contrary.

But not only have hydropathists despised the discrimination usually employed in the selection of cases for cold bathing, they have manifested an equal apparent boldness in the manner of using it. In place of the spongings and the dribblings to which ordinary practitioners commonly deem it prudent to limit the use of this remedy, they employ active plunging and powerful douches. Perhaps it is to this that they owe some portion of the impunity with which they appear to have applied it so generally. They assert that the more violent practice is really the more safe, and that the danger to be apprehended is in proportion to the supposed mildness of the process, sponging being less safe than total immersion, and a shower-bath more dangerous than a douche. And assuredly theory, in this respect, goes with them to some extent. In the plunge there is a sudden shock, which awakens nervous energy, and leads to speedy and effectual reaction; whereas, in sponging, the whole surface is exposed to a gradual and powerful cooling, without the protection of stimulus. In the former, the whole frame is at once covered with water, and shielded from the reducing evaporation which would attend the latter. Moreover, the plunge can be more speedily gone through, and followed up more immediately by exercise. The same distinction may be made between the hydropathic douche and the orthodox shower-bath. The force of the latter falls almost exclusively on the head and shoulders, as it merely trickles down the rest of the frame. How different is this from the powerful impulse of the douche upon all the muscular parts!

9. Another conspicuous item in the catalogue of hydropathic machinery is the *sweating process*. On this subject hydropathists are, in some measure, divided. It is said that Preissnitz has considerably modified his views respecting its efficacy and its safety. In the earlier period of his practice he seems to have employed it in nearly all cases. More recently he is said to have discarded it, as a general remedy, in favour of packing in the wet sheet, though still largely applying it in cases to which his matured experience has taught him to regard it as especially beneficial. We wish here to direct attention to it merely in a physiological and pathological point of view, and need not, therefore, enter into the question as to the relative value of the past and present practice of Preissnitz.

The skin is a part through which nature has arranged that a large amount of matter should be removed from the body during health, and a still larger amount, of different character, in the process of recovery from many diseases. It is well known that a deficient cutaneous excretion is incompatible with perfect health. Perhaps there is scarcely any disease in which the function of the skin is not, to some extent, deranged. To what extent, physicians have not bestowed sufficient pains to learn; nor have they been accustomed to give much attention to this part, in the practical investigation of diseases. Still less has it acquired an important position in the list of parts to which medical treatment is directed. Therefore, we possess little information, in medical writers, as to the amount or frequency of cutaneous disturbance in general disease, as to the effect of therapeutic means in correcting such disturbance, or as to the value of the correction in the cure of disease.

According to Preissnitzian writers, in almost all cases of indigestion, gout, rheumatism, nervous affections, indeed of chronic disorders in general, the action of the skin is either deficient or depraved, the part itself being found dry, hard, rough, thick, pale, relaxed, or in some other manner unnatural. They farther tell us that a course of perspiration, or of the wet sheet, followed by cold bathing, corrects these signs of disorder, and reduces the part to its normal condition; and that the beneficial influence of the remedy is speedily manifested in the improvement of the case in other respects. But it might be expected that such a course would, at least, reduce the general strength, and require more vigour of constitution than many such patients possess. And yet, if we may believe the hydropathists, or even their patients, a course of active hydropathic sweating is found to strengthen, instead of weakening, the system. There is a gain, instead of a loss, of weight under its operation. Whether this be attributable to the subsequent cold bathing, to the water drinking, or to the peculiar regimen, may be a matter of question; but the fact would seem to be too notorious to be contradicted. We are told that it is no infrequent occurrence at hydropathic establishments for the liquid perspiration to be streaming on the floor, having penetrated through the material on which the patient is reclining, as well as the blanket in which he is wrapped! The blanket also, when removed from the person, is dripping with liquid in all directions, as if itself just removed from the bath! On these occasions several pounds of matter must be removed from the body. The patient, dripping and steaming, next hastens into the plunge bath, stays there his appointed time, undergoes the prescribed friction, drinks his water, and finds himself actually invigorated by the strange process he has undergone!

Nay, more: it is placed beyond doubt, by experience, that this proceeding may be repeated daily, or even twice a day, for *many months*, without producing any deleterious effect upon the general health? Many cases have occurred in which it has been ascertained that it has been attended with an increase of weight, and that of no slight amount. We know the particulars of one case, in which a gouty gentleman gained seven pounds in a fortnight of such treatment; and of another, in which there was a gain of eight pounds in ten days. We are also acquainted with the case of a lady who was unable to walk at any other period of the day, except immediately after the sweating process—a sure proof that it did not occasion debility.

The safety of the immediate succession of cold bathing upon copious sweating has been called in question; but the practice of so many hydropathists as there are around us amply establishes this point. On scientific grounds the question was completely set at rest by Dr. Currie.

An effective and innocuous means of increasing the excretion from the skin being thus found, which appears to combine with its own peculiar action the indirect effect of a tonic, have we not reason to regard it as a promising instrument of cure, in many disordered states of the system? We fully believe that we have. We know the utility of augmenting the secretion of the mucous membranes, the liver, the kidneys; we recognize this in our constant practice. It is by this means that we combat a large proportion of chronic as well as acute maladies. Why should the skin alone be neglected? Physiology teaches us that it is the vehicle for conveying out of the system a large amount of matter, as well solid as liquid; and practical experience exhibits it as the channel through which the *materies morbi* in many instances, and the burthen of plethora generally, find their exit. These facts indicate it as a legitimate locality for the same artificial measures which are found serviceable on other secreting organs.

It may be objected to what we are now urging, that profuse perspiration itself characterizes many diseases, of which it is one of the most formidable symptoms. How can sweating cure acute rheumatism, it may be asked, of which it is almost a constant feature? But the same remark applies to other medical phenomena. Excessive purging and increased action of the kidneys are dangerous, frequently mortal, symptoms. But does that prevent our employing them as remedies? Do we not, in spite of our frequent experience of their injurious effects, apply them almost constantly to the cure of disease? Are there ten cases out of ten thousand in which some kind of purgatives are not administered? Nay, is not dysentery itself treated by purgative calomel? Let us extend the same tolerance to sweating. It is contrary to all the instruction of experience to confound the consequences of a phenomenon violently excited by morbid causes, with those it induces when seasonably created, and carefully managed, by skilful treatment.

In many of these cases, the benefit does not appear to result so much from stimulating the function of any particular organ, as from removing a certain portion of matter from the system at large. There is no reason to suppose that exciting the liver, the colon, the duodenum, or the kidneys, for instance, has any special influence over a morbid condition of the brain. We find that drugs which act upon any of these organs, frequently relieve such conditions, and they may often be selected indiscriminately, the one answering much the same purpose as the others. A common antibilious pill, retailed for a penny by a druggist, or a patented nostrum of Cockle or Morrison, will generally do as well as the most elaborate prescription. The particular adaptation seems to depend more on constitutional idiosyncrasy than on any fixed relation of the part diseased with the part treated. The whole of those remedies appear to act in such cases, either by a general principle of counter-irritation, or by removing a quantum of fluid, or of excretory matter, from the circulation, either of which object might be attained as speedily, as certainly, as extensively, and as safely, by the skin as by any other part.

10. But the power of the water-cure over excretions is not limited to the skin. It professes to be both a purgative and a diuretic. That it is diuretic, in a certain sense, needs no proof. It is no new discovery that, in proportion to the quantity of fluid imbibed by the mouth will be the quantity emitted by the kidneys. This, though verbally, is not medically a diuretic action. It may consist simply in the mechanical discharge of the fluid imbibed, with no augmentation of the proper functions of the kidneys, as respects the previous condition of the blood-vessels. But it is not perhaps unphilosophical to give hydropathists the benefit of supposing that water-drinking may do indirectly what it does not appear to do directly: by its diluting power, may it not destroy the influence of any mischievous constituent of the blood, the excess of dilution being immediately repaired by the removal of the water through the kidneys, in company with the deleterious matter dissolved in it? This view might be admitted, if it could be shown, practically, that drinking water has the same effect on disease as taking diuretics.

The purgative action of hydropathy is less equivocal. It frequently happens, in cases of constipation, that, after a few days' or weeks' use of its appliances, the patient is attacked with diarrhea. This is sometimes troublesome, but we believe seldom dangerous. On its subsiding, the bowels are said to have generally acquired a regular and healthy action, which is thenceforward maintained by persevering in the drinking, bathing, &c. In other cases, a regular action of the bowels comes on in a gradual manner, without the occurrence of diarrhea, the treatment appearing to influence the bowels through its action on the system at large. In others, and every one has seen examples of this, the mere drinking of a few glasses of water before breakfast is represented as a purgative that may be relied on. In these the daily dose is regulated according to circumstances, being increased when signs of torpidity are observed. We are ourselves acquainted with some persons who regulate this function as accurately by water-drinking as they formerly did by medicinal aperients. There are cases, again, in which the sitz-bath, or other external applications of cold water, produce a purgative effect.

It may be asked, is not this effect too uniform for the purpose of the practical physician? Does it not often result from the mere percolation of water through the mucous lining of the intestinal canal? Is it not, therefore, a mere pouring out of what has been swallowed? Is it not clearly inadequate to excite the particular action of the liver, the pancreas, the lower or upper portion of the intestinal tube? Is it not necessary that we should be able to act on these parts separately, for the effectual cure of disease? These questions, important as they appear, may with equal justice be asked as to the practical proceedings of our profession in general. It is true that, in theory, many nice distinctions are laid down respecting the peculiar operation, as to locality or otherwise, of different cathartics. But are these distinctions generally observed in practice? Did not Abernethy's page 72 contain the curative maxim for all cases? and were not his prescriptions almost always identical? Has not every respectable family doctor his "my pills," carefully prepared from the same ingredients for every difficulty in the bowels? Is not the the black draught as universal a purgative as Preissnitz would make cold water? Are not all our moneyed dyspeptics and hypochondriac nabobs sent in a body to mineral springs, because they are purgative, without any preliminary investigation as to their action on the duodenum or the colon, the liver or the pancreas, or as to the expediency of such action in the individual case in question?

11. We observe, also, in the history of hydropathic practice, the development of a peculiar sedative or tranquillising influence. It is well illustrated in the following passage from Mr. Mayo's preface:

> "Through repeated attacks of a sort of rheumatism my constitution appeared completely broken down. Already crippled in my limbs, *preserving what power of exertion I still retained only through the use of opium*, and my indisposition still increasing, I looked forward to being before long worn out with suffering—as to death, as a release. I could not bear the fatigue of a land journey, or I should have gone at once to Graeffenberg; but Coblentz and Boppard might be reached from London by water—so I went to Marienburg in June, 1842. On arriving there I was placed on a routine system of sweating and bathing. The immediate effect on my health was strikingly beneficial, *and in a week I was able to relinquish the use of opium.* The rheumatism did not, however, give way proportionably to my general improvement. The pains of the joints were, indeed heightened." (p. 1.)

This was a painfully severe case, one in which every conceivable remedy had been previously tried, not excepting repeated change of air, the Bath-waters, &c.; yet nothing had succeeded in relieving the system from the necessity of constantly using opium. A "routine system of sweating and bathing" was applied, and in a week the patient was able to relinquish his doses of opium, notwithstanding that the rheumatism did not give way; indeed, the pains in the joints increased. How is this to be explained? only by supposing that, independent of any curative influence over the actual disease, the water-cure exercised some sort of sedative action on the system at large. Similar instances are said to be familiar at hydropathic establishments. If these accounts may be depended on, hydropathy would appear to contain, in its armamentarium even an anodyne, and one of great power. Every practitioner knows the difficulty presented in the treatment of chronic cases, by morbid irritability, and painful nervous sensations, which are not only intolerable to the patient himself, but most prejudicial to his recovery; and which can only be relieved, from time to time, by repeated and gradually augmented doses of a drug, whose own effects are almost as pernicious as the symptoms it is used to palliate. This is one instance of a predicament in which the physician is not infrequently placed, when he has most gravely to consider whether there is most mischief in the disease to be combated, or in the only remedy by which it can be encountered. If "a routine system of sweating and bathing" affords a means of extrication from the present instance of this difficulty, this is a strong reason why it should not continue to be obstinately excluded from the well-fenced pale of the medical profession.

12. In addition to the effects already considered, and which have occupied as much as can be spared of our space, the water-cure pretends to the possession of other important powers. Thus, it is said to be a *stomachic*, since it almost invariably increases the appetite. It is a local *calefacient*, in the application of the wet cloth covered by the dry one. It is a *derivative*, cold friction at one part, by exciting increased action there, producing corresponding diminution elsewhere. It is a local as well as general *counter-irritant*, the compress frequently acting, if not like a blister, at least, like a mustard poultice. It is essentially *alterative* in the continued removal of old matter by sweating, and its renewal as shown in the maintenance of the same weight.

13. Lastly, our subject brings us to make a few remarks on *medical habits* in reference to chronic cases. In such cases, we have only commenced the treatment, when we have removed the immediate symptoms; the real difficulty consists in preventing their recurrence. Accordingly, the patient quits his physician with ample instructions for his future guidance, and with most impressive warnings as to perseverance in their observance. What are these instructions, and to what habits do they lead? Let us take a case of "biliousness" or chronic dyspepsy, and briefly trace the history of its "legitimate" treatment, according to the heroic school of London.

In addition to constipation, the patient, we shall suppose, is affected with acidity, deficient or depraved appetite, foul tongue, oppression after meals, susceptibility to cold, debility, headache, despondency, irritability of temper, inconstancy of purpose, hopelessness of relief, with divers local grievances. A few brisk cathartic doses, combined with mutton diet, and a gentle stimulant, empty the bowels, and carry off most of the attendant ills. By continuing this plan for a short time the patient is, what is medically termed, cured; but, for future protection, he is furnished with a prescription—say—of aloes, colocynth, and calomel, or some such compound, to take *pro re natâ;* another of senna and salts, to take less frequently, as more urgent symptoms require; a third of calumba, gentian, or cinchona, to take at noon with a glass of sherry. He is told to live on boiled mutton, rice, and dry bread, avoiding fruit and vegetables.

What future, as respects health, has such a person before him? As long as he lives he will be a martyr to the disease, probably in an increasing degree; he must abandon all hope of the action of the bowels ever resuming its normal state; his general strength will gradually diminish; his nervous system will become more and more irritable; his whole comfort and enjoyment will be sacrificed in order to empty the alimentary canal; he will become one of the most pitiable of all sufferers, a person "living by rule;" his health will be supported, as one of our witty doctors remarks, like a shuttlecock between two battledoors, by the alternate impulse of senna and sherry, of calomel and coffee, of jalap and gentian. As long as these instruments are so directed, that their respective influences succeed each other in compensating proportion, all seems, for the time, smooth; but let either overdo or underdo the mark, and every thing breaks down. The game must then be commenced anew, to be continued as long as feather and cork resist the tendency which it has to knock them to pieces.

This is scarcely a caricatured picture of the discipline to which dyspeptic patients are often forced to submit. Everybody's experience must furnish abundant proof that the illustration is too close to nature. It is in the latter stages of these affections, when the patients have long been under the influence of therapeutic means, that Preissnitz pronounces them "drug-diseases." If, by this term, he means that drugs constitute the whole disease, then he is no doubt wrong; yet, in one point of view, he is right. The original complaint for which the drugs were administered might, very probably, have been one requiring some artificial remedy, and which would have induced more serious consequences, had not some such remedy been employed. But it is quite possible that a persevering use of such remedies may create a train of symptoms, in addition to those which existed before, and induce such a host of wants as may constitute a prominent feature of the case, by the time it is submitted to the curative process of such a practitioner as Preissnitz; therefore his term, drug-disease, may not be altogether inapplicable.

But what is often the result of placing the cases, now under consideration, in a hydropathic establishment? Precisely such as might be expected from the abandonment of a pernicious custom, and the adoption, at the same time, of a more natural mode of life with healthier and hardier habits; and with the immense additional mental stimulus of cheerfulness, of faith and hope in the new system, and of unbounded confidence in the new doctor. It is accordingly the general report that, in a large proportion of such cases, the patients are enabled immediately to discontinue the use of purgative medicines; they can bear a mixed animal and vegetable diet, in the ordinary proportion; a regular action of the bowels is shortly acquired, and no further stimulant or pharmaceutical tonic is necessary. When they quit the establishment, formal and complex means being no longer required, we are assured that they are able, for a time, at least, to maintain the ground gained, simply by common-sense diet, drinking a few glasses of water in the morning, taking a daily cold-bath, and persevering in their habitual exercise. The country rings with such accounts as these; if they are correct, undoubtedly, the patients are in a fair way of recovering their lost health and strength, and are pursuing, subsequently to systematic treatment, a much more rational and scientific course of medical habits, than that enjoined to the dyspeptic disciple of medical orthodoxy.

The questions, with which we set out, may now be hypothetically answered: they were, "Does hydropathy furnish the physician with instruments which he, as a skilful workman, can undertake to employ? Does it contain, among its various machinery, any really therapeutic means, any properties capable of carrying out the indications which we regard as palpable in many diseases?" These questions, we think, may be allowed to have been answered in the affirmative, if we may depend on the results of our own limited experience; they must be allowed to be so answered, and unequivocally, if we may admit as perfectly trustworthy, the accounts published by the hydropathists themselves, and by those who have subjected themselves to the treatment. On another occasion we may, perhaps, endeavour to sift this evidence in a more rigid manner, in order to ascertain, with certainty, what in it is true, what false, what doubtful, and what inapplicable. But in any inquiry we may institute, we must continue to examine the water-cure relatively to other modes of practice: this is the only method of arriving at an estimate of its actual value to the practical physician. The imperfections which it shares in common with ordinary treatment, and which are inseparable from all human performances, may be left entirely out of sight; to dwell on them, would be uselessly to encumber the question, like inserting a crowd of corresponding items upon both sides of an equation. The philosopher's duty is to remove such superfluities, in order that the real problem may appear in a just and intelligible form.

In conclusion, we will venture to place on record the following, as among the more important impressions which have remained on our mind after a careful examination of the whole subject:

1. We should be glad to see Dr. Currie's practice revived (for the sake of experiment, at least,) in all its boldness, for the suppression of the general febrile paroxysm. On carefully looking over the evidence published by Dr. Currie and his contemporaries, it is impossible to deny that they attained a larger amount of success in treating fever by water than other practitioners have done by other means. We have already pointed

out how their practice has been misunderstood by modern writers. But, while we regard this practice as well adapted for treating general fever, we find no proof that it is competent to meet the dangerous local complications with which fever is so often accompanied. These complications may reasonably be expected less frequently, when the early treatment of fever is rendered more efficacious. But, when they do occur, we find nothing in hydropathic writers to show that lancets, leeches, blisters, &c., can be dispensed with.

2. In a large proportion of cases of gout and rheumatism the water-cure seems to be extremely efficacious. After the evidence in its favor accessible to every body, we think medical men can hardly be justified in omitting—in a certain proportion of cases, at least—a full trial of it. No evidence exists of any special risk from the water-practice in such cases.

3. In that very large class of cases of complex disease, usually known under the name of chronic dyspepsia, in which other modes of treatment have failed or been only partially successful, the practice of Preissnitz is well deserving of trial.

4. In many chronic nervous affections and general debility we should anticipate great benefits from this system.

5. In chronic diarrhea, dysentery, and hemorrhoids, the sitz-bath appears to be frequently an effectual remedy.

6. We find nothing to forbid a cautious use of drugs in combination with hydropathic measures. On the contrary, we are convinced that a judicious combination of the two is the best means of obtaining the full benefit of each. The water-cure contains no substitute for the lancet, active purging, and many other means necessary for the relief of sudden and dangerous local maladies. The banishment of drugs from his practice was necessary, and perhaps natural, on the part of Preissnitz: the like proceeding on the part of qualified medical men superintending water-establishments in this country, evinces ignorance or charlatanry, or both.

7. With careful and discreet management, in the hands of a properly qualified medical practitioner, the water-cure is very rarely attended with danger.

8. Many of the principal advantages of hydropathy may be obtained in a private residence, with the assistance of ordinary moveable baths. Therefore, it can easily be brought under the direction of the regular medical practitioner.

9. In many cases, however, it is evident that what may be termed the mere *accessories* of the water-cure, are of extreme importance in bringing about a favorable result; and these accessories are frequently not available—or available in a very inferior degree—in ordinary practice. Among the more important of these accessories we may mention the following as having relation to most of the chronic cases treated in hydropathic establishments: 1, relief from mental labors of an exhausting or irritating kind, from the anxieties and responsibilities of business, from domestic irritations of various kinds, from mental inaction or ennui, &c.; 2, change of locality, air, scene, society, diet, &c.; 3, the fresh mental sti-

mulus involved in the almost constant occupation of the patient's time, in the performance of the numerous and various dabblings, paddlings, sweatings, washings, drinkings, rubbings, &c., imposed by the water treatment; 4, the frequent and regular bodily exercise taken in the open air, or within doors; 5, the powerful mental stimulus supplied by the confidence generally reposed by the patients in the means employed, and by the consequent hope, alacrity, cheerfulness, &c.; 6, the total abandonment of vinous and other stimulants, and of drugs,—all of which have, in a large proportion of cases, been tried and found, not only useless, but probably, productive of disadvantage.

10. A certain and not inconsiderable portion of the benefits derived from hydropathic establishments, are, however, attainable without them, by other means, as by travelling, &c. &c. For example, we suspect that many of the most striking results witnessed in such establishments, as in the case of Sir Edward Bulwer Lytton or Mr. Lane, would have probably been obtained, if the patients had chosen to hire themselves, and had worked as agricultural laborers, in a dry, healthy district, and had lived on agricultural fare, sufficiently nutritious in quantity and kind, for a sufficient length of time.

11. Notwithstanding the success of the founder of hydropathy, its practice by non-professional persons can neither be fully advantageous nor safe. At the same time, it is true that very little experience is necessary to enable an educated medical man to acquire sufficient insight into it for purposes of practice. Many of the best hydropathic physicians have, in the first instance, devoted very few weeks to studying the subject in Germany.

12. Many advantages would result from the subject being taken up by the medical profession. The evils and dangers of quackery would at once be removed from it. Its real merits would soon be known. The tonic portion of its measures might then be employed in conjunction with special remedies of more activity, which, no doubt, would often prove exceedingly beneficial.

13. The benefits ascribed to hydropathy, but arising indirectly from the abandonment of drugs, vinous and other stimulants, &c., may certainly be obtained without sending patients to Graeffenberg.

14. Finally, it must always be remembered that the distinction between quacks and respectable practitioners is one, not so much of remedies used, as of skill and honesty in using them. Therefore, let our orthodox brethren be especially anxious to establish and to widen, as far as possible, this distinction between themselves and all spurious pretenders. "*Artem medicam denique videmus, si à naturali philosophia destituatur, empiricorum praxi haud multum præstare. Medicina in philosophia non fundata, res infirma est.*"

Art. XI.

Redogörelse för Sjukvarden a Kongl. Seraphimer Lazarettets Afdelning för Invertes Sjuke, under Loppet af Ar 1842.

Medical Report of the Internal Diseases treated in the Royal Seraphim Hospital in Stockholm, during the Year 1842. By Dr. Magnus Huss. —*Stockholm*, 1844. 8vo, pp. 228.

We had imagined, on reading the title of this book, that it would be mere hospital report, full of long details of cases, but with little attempt at generalization, and, consequently, merely valuable as an addition to medical statistics. From the charge—often well-founded—of deficient knowledge of contemporary medical literature and science, we knew our author would be perfectly free; but we were not prepared to find, under the modest name of an Annual Report, such elaborate disquisitions on the origin and nature of some of the most obscure diseases;—and these written, too, with a candour and honesty of purpose, which, in our own insular pride, we rarely expect to meet with beyond the British seas.

Before examining this Report in detail, we may premise that it is only a portion or a single part of an elaborate series, several volumes of which are already before the Swedish public, though they are as yet unnoticed in our British Journals. Unfortunately these preceding volumes are not as yet in our possession, and this is the more to be regretted, as several maladies and classes of diseases which, to judge from the number of cases briefly alluded to in this volume, are of very frequent occurrence in the Swedish capital, obtain in this Report only the mere notice of their number, while, for details of their peculiarities, and for the author's opinions of their nature and treatment, we are referred to the preceding volumes.

In this Report of 1842, the various diseases that affect the human frame are considered successively under the following heads:

1. Diseases of the nervous system.
2. " respiratory organs.
3. " organs of circulation.
4. " lymphatic system.
5. " digestive organs.
6. " liver and of the spleen.
7. " urinary and genital organs.
8. " organs of locomotion.
9. " skin.
10. Mental disorders.
11. Cases of poisoning.

In the clinical wards of the Swedish hospital the most important diseases are necessarily selected for the instruction of the pupils, but those also of less intensity but of more frequent occurrence, are admitted to familiarize the student with the aspect of maladies most common in his future practice. During the year 1842 seventeen students were admitted as clerks in the wards (tjenstgörande), and to each of these clerks were allotted, in the space of nine months, 35 cases of disease, of all of which they were obliged to keep a full and complete journal for the inspection of the clinical professor. Besides the clinical observations delivered by the professor at the bed-side, a course of special pathology is continued throughout the session in lectures which are given twice in the week; and we can estimate the comprehensive nature of this course from the fact, that the nine months of the past session were exclusively occupied by the consideration of diseases of the intestinal canal, so that two or even three years must elapse, ere all the maladies of the human frame are completely described. The mortality in the hospital, during 1842, was 11½ per cent.; the greatest number of patients died in the earlier months of that year; while December presented the lowest rate of deaths.

No epidemic of any consequence appeared in the capital during this year, excepting a gastric nervous fever among the police or gendarmerie, which increased the usual average of that complaint by about 60 cases.

The first division, viz. that of diseases of the nervous system, occupies a very large portion of this volume; but we do not regret to find more than one hundred pages devoted to these interesting but most obscure maladies. Our author commences with inflammation of the brain and spinal cord, and of their envelopes. The first case, which is related at great length, is one of fatal inflammation of the spinal meninges. The patient, a sailor, was able to perform his usual duties for fourteen days after the accident, before any serious symptoms supervened. No disease was discovered on dissection of the brain after death, but there was slight delirium during life, with great irritability of temper. Convulsions occurred, both in the upper and in the lower limbs, and it was necessary to use the catheter, as the bladder did not act. From the appearances recorded on the post-mortem examination, it would seem that the dura mater was the original seat of the inflammation, and that pus had exuded from thence into the neighbouring muscles on the back; though, during life, the latter phenomenon could not be distinguished from a simple abscess, the result of supposed external injuries. Dr. Huss ascribes the delirium and irritability of temper in this patient, to sympathetic irritation of the cerebral system. Death ensued in the fourth week after the accident.

The second case is not in itself remarkable, being one of simple *meningitis cerebralis*, but it was accompanied with a general emphysema of the whole trunk, which came on on the twenty-second day of the disease: no cause for this appearance could be discovered on dissection. Dr. Huss aptly compares this emphysema to that which occurs in typhoid fever of a malignant character, where the intestines become enormously distended with flatus. The cause of this symptom may, he thinks, in both cases, be traced to diminished energy of the inflamed brain, whereby the blood becomes deteriorated, and tends to separate into its constituent parts. Our, author, however, evidently does not mean to infer that, in all cases of typhoid fever with great distention of the bowels, there is necessarily inflammation of the brain or of its envelopes.

In the third case of this Report (p. 9) we have an illustration of the great advantages of full doses of calomel after venesection in acute cerebral meningitis. The treatment here pursued seems to have been most energetic and sensible; it is one of those cases, where no doubt can be raised, as to the powers of medical science in arresting disease.

It is justly remarked by Dr. Huss, in reference to this case, that we must not be deterred by the apparent debility or defective nervous energy, from pursuing a steady antiphlogistic course of treatment. "It is this horror of debility," says he, "which too often paralyses the efforts of the physician, and allows the inflammation to proceed unchecked to a fatal termination." The great value of free evacuation of the bowels in cerebral disease, appears to be better known to Dr. Huss than to many of his continental brethren. "Should calomel," he says, "fail of purging, I assist its operation by purgative enemata, and by the free use of croton oil, or of turpentine." Our author has seen little benefit from the cold douche applied to the head in cerebral inflammation, but he acknowledges its marked efficacy in the delirium of typhus fever. At the same time he admits that there is great difficulty in the diagnosis between the two affections, as the blood often presents the same disintegrated condition in cerebral inflammation, as in the most malignant typhus. Blisters to the shaven scalp he unequivocally condemns; in all cases in which they have been tried, he has found them to do harm by increasing rather than diminishing the symptoms; though in the latter stages, when inflammation has subsided and effusion still remains, they may occasionally be useful. "When," he continues, "the symptoms of inflammation and congestion have subsided, and are succeeded by those indicating debility of the brain and of the nervous system, I find it best to wait a day or two, till there is an obvious indication for the use of stimulants and sedatives; I then employ camphor and arnica to fulfil the former intention; and opium seems to be best adapted for the latter. I have more than once observed, that opium in doses of one or two grains when muttering delirium succeeds to violent nervous excitement, has quickly produced refreshing sleep." English practitioners will fully appreciate the truth of this observation.

Two cases of *hydrocephalus internus*, the next disease here mentioned, are not of remarkable interest, except that the first was found to be accompanied by a large cavity in the cerebellum containing fluid, and totally distinct from that found in the ventricles. He compares this appearance to the sacculated dropsies of the ovary, and believes these cysts to be formed by a defect in development of the cells of the cerebral substance. Dr. Huss has never observed any connexion between disease of the cerebellum, and the excitement or depression of the sexual appetite.

Apoplexy. Of 17 cases, 12 occurred in males, and 5 in females; and nine of these patients were between their thirtieth and fortieth year, which is surely a greater proportion than that met with at the same period of life in this country. Of these 17 cases, 8 proved fatal; in the preceding year only 4 died out of 15.

Our author has seldom found blisters or irritant applications to the scalp of any use after apoplexy, and the possible benefit they may confer is, he thinks, more than counterbalanced by the annoyance they occasion to the patient.

Dr. Huss does not believe that, when the power of speech is entirely lost in apoplexy, the effusion must necessarily have taken place in the anterior lobes of the brain; he has seen speech wanting when the posterior lobes alone were the seat of the maladies. In case 8 the apoplectic effusion occurred in the posterior lobes of the left side, and the power of speech was entirely absent during the six months that the patient survived the attack; but we must observe that the left hypoglossal nerve was found to be only half the size of that upon the opposite side.

Our author is of opinion that, the larger the clot and effusion of blood in apoplexy, the greater will be the loss of motor power, but he acknowledges that the same does not hold good in regard to the seat of the lesion; but, he adds, it is well known that the case is different in apoplexy occurring at the base of the brain, where a very small effusion causes great loss of nervous power.

At page 30 of the Report we find an interesting case exceedingly well detailed, which, during life, was supposed to be apoplectic effusion into the pons varolii. There was paralysis of both sides of the trunk, with partial loss of feeling, but the intelligence remained perfect. After death an effusion of blood was found in the left crus cerebri, and another of like character in the corpus striatum of that side. Dr. Huss owns that he dares not venture on any explanation of the double paralysis in this case.

Before concluding this portion of our subject we may observe that our author throws out a suggestion, that an intimate connexion will, one day or other, be discovered between lesion of the corpora striata and optic thalami, and the functions of the urinary and genital organs.

Under the title of, *Organic lesions of the brain* (p. 33), we find a case of supposed softening of that organ, with paralysis of the right side, spontaneous movements of the whole of the extremities, and loss of feeling in the fingers and toes. Two grains of opium divided into four doses were given daily, with ten grains of the "sal cornu cervi," by which combination our author believes that the narcotic properties of the opium are diminished, leaving its sedative qualities untouched. Dr. Huss has found opium very useful in cases of suspected softening of the brain.

> "I have constantly observed," he says, "that when in this complaint the most prominent symptoms were cramps of the limbs, or tremors, or both of these symptoms together, opium was the most efficacious remedy, though I cannot venture to assert that it effected a cure." (p. 37.)

Osteoma frontalis. In speaking of this disease and of tumours in general, pressing upon the surface of the brain, Dr. Huss observes, that it is difficult to decide *after death* what portion of the cerebral system has particularly and secondarily suffered from such pressure, though it is easy to determine what convolutions have been immediately compressed. The case of frontal osteoma here described, closely resembles those cancerous tumours described by Walther and Cruveilhier, and so beautifully figured by the latter. The pulsations of the brain could be very distinctly heard on applying the stethoscope over the tumour. How far the congested and enlarged vessels in the tumour itself may have contributed to the sound of cerebral pulsation, is a question which we leave to the consideration of our readers. It may be doubted, perhaps, in the present instance, whether we are justified in identifying the tumour described by Dr. Huss with those of a decidedly cancerous character; for its external portion diminished, in a very marked degree, under the prolonged use of mercury and iodine; while, at the same time, the mental powers which were before much weakened, regained their original force. The mode of treatment was, by first blistering the tumour and then applying the unguent. hydrarg. iodid. The tumour did not entirely disappear, but Dr. Huss suspects, from the complete return of the mental faculties, that the internal portion of it, which produced all the evil symptoms, was that which was soonest and most completely removed.

Spinal Irritation. To the consideration of this disorder Dr. Huss devotes a very considerable portion of this volume. It is indeed a malady concerning which much has been written lately, but we have seldom or never met with a more complete history of its varied symptoms than that contained in the pages of this Report. Our author complains of the numerous, varied, and complex definitions attempted of this disease; his own, however, will probably admit of dispute. He believes, "that that same condition of irritation which, physiologically speaking and under physiological influences, is transplanted from impressions made on the surface-nerves of the body to the spinal cord, here appears as a disease arising primitively from the spinal cord itself, and which is thence propagated to the external surface, or extremities of the nerves." Physiological irritation, therefore, is transmitted from without to the nervous centres, pathological irritation from the centres to the extremities of the nerves.

Dr. Huss asks, if this be not an old disease under a new name? or at least he acknowledges, that were many of the symptoms to be taken separately they have undoubtedly been before described; but the honour of uniting them all as one malady is due only to the present day. The subject of spinal irritation has been most ably discussed by English writers; indeed we may justly assume to ourselves much of the light that has been recently thrown on this obscure disease. The labours of the Messrs. Griffin, of Limerick, in this regard, are pleasingly acknowledged by Dr. Huss, who seems also to be well acquainted with the investigations of Dr. Marshall Hall, of Dr. Copland, and of Mr. Travers, as to the connexion of reflex action with the phenomena of the disease in question. Nor have our German brethren been slow to pursue a subject so congenial to their laborious and speculative minds. Our author has, we think, steered a middle course, a thoroughly practical one, between the mazes of German speculation and the hasty conclusions too often arrived at by the clever writers of France. We do not think we say too much when we state his views and practice here will be found as consonant to English feelings and to English medicine, as is the construction of the beautiful language of Sweden to that of our mother country.

Dr. Huss objects to the generally received definition of spinal irritation, viz. that it is an alteration in the *functions* of the spinal cord, independent of any organic change in that part; for he observes, that this merely expresses the condition and not the original cause of the malady; pathological anatomy, he acknowledges, has taught us but little regarding its origin, and that little merely of a negative character, viz. that the same phenomena which are developed in organic disease of the spinal cord, may also appear during life, when no lesion or change of structure can be discovered on dissection.

The symptoms of spinal irritation are classed by our author under the following heads:

1. Pain of various parts of the vertebral column, existing either idiopathically or developed by pressure. 2. Cramps, either of a tonic or of a clonic nature, in those parts subjected to the influence of the spinal cord. 3. Loss of power in the same portions of the body, varying from simple stiffness and weakness to complete paralysis. 4. Altered sensibility, either by excess or by great diminution of sensation.

Of all these phenomena the varieties seem to be almost endless; and not less remarkable is the constant variation of symptoms in the same individual. We will, however, briefly consider each of these divisions as here made out more in detail.

1. Dr. Huss believes that the pain developed by pressure on the vertebræ is of more importance, in a diagnostic point of view, than that experienced by the patient without such manipulation; but he differs from some of our recent writers, when he asserts that it is only the feeling of pain developed by *firm* pressure that is of importance. With the Drs. Griffin he agrees that pain *above* the vertebræ, subjected to pressure, is rarely observed. It is almost always felt in those organs or parts of the frame that correspond in position to the compressed vertebræ; or, if not there, it occurs in more distant parts lying *below* the seat of pressure. But Dr. Huss does not maintain that tenderness on pressure of the vertebræ must necessarily exist in spinal irritation. On the contrary, it is occasionally absent; and its value as a diagnostic sign is considerably diminished by its occurring, with a similar train of symptoms, in organic disease of the spine itself. As to the immediate cause and nature of this symptom, Dr. Huss rejects alike the theory of its being occasioned by pressure on the spinal cord through the medium of the vertebræ, which is anatomically impossible, as also the doctrine of Stilling and of Hirsch, who believe the seat of the pain to be in the muscles, skin, and ligaments interposed between the vertebræ and the hand of the compressor. Our author finds no other means of explaining this symptom than by supposing that there exists in such cases a neuralgia of the vertebræ themselves. This theory may perhaps suffice in the present state of our knowledge to account for such intense pain being felt on firm pressure of the vertebræ; nor do we think it inadequate to explain those cases where the slightest touch occasions agonizing torture, and even convulsions. The theory of a neuralgic condition of the sensitive surface-nerves of the back must be also allowed to carry with it a considerable semblance of truth.

2. "Phenomena resembling cramp," &c. From the simplest attacks of cramp these symptoms may pass into severe convulsive movements of the extremities, closely resembling those of chorea; which disease our author considers to be spinal irritation, in its most complete condition, with clonic convulsive movements. At other times the symptoms are rather those of tetanus; and this latter malady is, according to Dr. Huss, the type of spinal irritation in its tonic and most acute form.

3. "Diminished power of motion." General paralysis of the motor powers sometimes occurs under this form.

4. "Altered sensibility," &c. This may be diminished, or much increased. In exalted sensibility, accompanying spinal irritation, the cutaneous nerves are often so affected in very limited portions of the surface;

sometimes this does not exceed the space covered by a shilling. Dr. Huss looks upon spinal neuralgia as another variety of this form. On the other hand, sensation may be totally absent from some portions of the cutaneous surface, even to impressions of the most painful nature.

Of the difficulties attending the diagnosis of spinal irritation our author speaks most feelingly; and he acknowledges that in many cases of obscure disease affecting the spinal column, it is necessary to suspend your opinion for weeks, nay, sometimes even for months, to enable you to decide on the nature of the malady, from the effects, or from the negative results, of the remedies employed.

Dr. Huss adds but little to our previous information respecting the causes of this malady, but ascribes its prevalence among the females of the higher classes in Sweden to that false system of female education, which neglects the due exercising of the body, while it overstrains the mental powers. Whatever, then, diminishes the bodily strength, as overstudy, excitement, or long and debilitating disorders, will predispose to this disease; especially in nervous and irritable constitutions. Of immediate exciting causes our author reckons the following: (*a*) Contusions, strains of the spinal column, from lifting of heavy weights; (*b*) intense mental affections, fear, sorrow, &c.; (*c*) lesions and irritations of the extremities of nerves springing from the spinal column, both of those distributed on the cutaneous surface, and those which terminate in internal organs, as also the presence of worms in the intestinal canal; (*d*) the most important and most frequent cause of spinal irritation is a disordered state of the blood itself (giving rise to the neuræmia of Dr. Laycock). As an example of this, Dr. Huss refers to the frequent occurrence of spinal irritation in chlorotic females, and to the phenomena produced by poisoning with strychnia, or secale cornutum, or by the introduction of lead into the system.

As regards the termination of this malady, it may persist for years without any advance towards a cure, the only change being the constant variation of the symptoms; but it may also eventually produce organic disease. Thus constant palpitation of the heart, from spinal irritation, may eventually cause the dilatation of that organ, or more frequently hypertrophy, with or without dilatation. With our English writers, Dr. Huss agrees that spinal irritation of itself is rarely fatal.

A long paragraph is devoted by our author to a review of the theories of spinal irritation. All that have hitherto been broached he rejects as doubtful and unsatisfactory, but modestly enough he does not attempt to replace them by a theory of his own. Perhaps he inclines a little towards the theory of Hirsch, whose opinions in regard to the minute anatomy of the nervous system are somewhat analogous to those of Dr. Stark, of Edinburgh. Hirsch believes the nerves to be tubes containing a fluid which, when collected in excess in any part of the nervous system, gives rise to pain, and to the other symptoms of spinal irritation; thus assimilating these phenomena closely to congestion of the blood-vessels. But, as Dr. Huss very justly adds, " until the existence of this nerve-fluid, or ether, or essence, can be distinctly proved, these doctrines may amuse but cannot instruct the scientific pathologist.

Treatment. External remedies should be applied in general to that part of the spine where pain is felt on pressure. Venesection, whether local or general, should be cautiously employed in debilitated persons, where the disease, as in chlorosis, appears to be connected with deterioration of the blood; and Dr. Huss remarks that venesection alone is seldom able to effect a cure, but at the most acts only as a palliative of the disease. Of the class of derivatives, Dr. Huss employs most of those generally used in English practice, recommending always that we should begin with the milder forms, while in protracted cases he has employed moxas and the actual cautery with success. "Should the patient seem worse after using the milder derivatives, I always abstain from having recourse to those of a severer kind."

Much alleviation of the pain is often obtained by simple manipulation or shampooing of the affected parts; and our author has also seen great benefit to result from covering the spine with wool or cotton dipped in turpentine. This is kept applied to the back by means of a sheet or towel, and its moisture is preserved by continual irrigation with fresh turpentine.

This plan is, however, not without danger in highly irritable constitutions. Mercurial ointment, pushed to salivation, has proved occasionally useful in our author's hands, though no inflammatory symptoms were present. Though no disciple of Preissnitz, Dr. Huss states that he has seen favorable results from the cold douche, and from cloths steeped in cold water, and applied to the spine; but he acknowledges that in the majority of instances these remedies have proved injurious.

Electricity and electro-galvanism are contraindicated, he thinks, in all cases where plethora exists, but may be employed with advantage in chronic and debilitated cases, though the advantage resulting from their use is so slowly obtained, as to render their application most tedious to the patient. In cases of a neuralgic character, magneto-electricity combined with acupuncture caused a most marked improvement in the symptoms.

1. *General external remedies.* "Bathing," &c. The warm bath is occasionally useful, but is much more efficacious when ʒj of caustic potash is diffused in the water. Perhaps, however, as our author suggests, a smaller quantity of the alkali than that above stated should be first tried, and the baths should be repeated at first every third, and afterwards every second, day. The mud-baths of Germany (soolenbäder) are often really valuable in cases of spinal irritation; and Dr. Huss deplores the little attention paid in general to the baths and mineral springs of his own country, which, he believes, would be found to be as efficacious as many of the celebrated German spas, if his countrymen would but be convinced that the means of cure were to be found at their own doors.

Clothing. Much harm is done, according to our author, by the habit of wearing excessive quantities of warm clothing. Thus he relates an instance of a lady, who suffered from spinal irritation, with constant chilliness of the back and abdomen, and who wore, besides two thick belts (maggördlar), not less than seven heavy shawls. A complete cure was obtained after she had gradually laid aside this excess of clothing, and had accustomed herself to sponging of the trunk with ice-cold water. A novel popular remedy is here introduced to our notice, viz. a bath in the freshlyplucked leaves of the birch tree, until perspiration breaks out over the whole body. Such popular remedies often succeed, when medical treatment has altogether failed. In the local palsies, so common in spinal irritation, Professor Huss has freely employed the endermic method, using strychnia in preference to any other remedy.

Internal remedies. Of antiphlogistics our author has only used mercury and tartar emetic, and these solely in plethoric subjects. The former was occasionally useful when pushed to salivation, but in many instances it utterly failed of effect. *Purgatives* he thinks absolutely necessary in most cases, to relieve the constipation and fæcal accumulation so common in females labouring under this disease, but those of the more drastic kind are decidedly to be rejected. The laxative mineral waters are, of all others, perhaps the most efficacious, though, for obvious reasons, they produce more benefit when the patient journeys to the springs. Much reliance is placed by Dr. Huss on *Tonics*, especially after fæcal accumulations have been removed, and of these he prefers the preparations of iron, while he considers quinine to have obtained an undeserved reputation in this disease. Many have regarded *narcotics* only as palliative remedies in spinal irritation. Our author is, however, deeply impressed with their great value as a means of cure. Small doses of opium or of morphia are decidedly the most successful means of treating the convulsive movements or tremors of the extremeties; while cramp of a more tonic character, resembling tetanus, is equally amenable to this powerful drug. It should be given in small doses at first, and be continued for at least a fortnight to effect a cure. In cases of paralysis, strychnia has seemed to be a very valuable remedy, when given in doses of $\frac{1}{24}$th to $\frac{1}{12}$th of a grain two or three times a day. In one case marked good effects were obtained by the use of the Faba Sti. Ignatii. Belladonna proved only a palliative in cases of spasmodic asthma, from spinal irritation. From the preparations of arsenic, zinc, and copper, Dr. Huss has obtained no satisfactory results.

As an antispasmodic, the preparations of valerian deserve, he thinks, their reputation. The root, or rather *the fibrils of the root*, of Artemisia vulgaris, which appear to be the only active part of the plant, are much lauded by Dr. Huss for antispasmodic qualities. The root itself, he says, is nearly, if not quite, inert. Arnica is also in Sweden, as in Germany, a favorite remedy, especially in debilitated constitutions. Phosphorus and musk are favorably mentioned by our author, in convulsive affections, especially in laryngismus stridulus.

Exercise and the due regulation of the diet are strongly insisted upon by Dr. Huss; and he warns us against yielding to the unwillingness of patients to move, or to rise from their beds; as the completion of the cure can often only be obtained by exercise of the affected parts. From page 65 to 109 of the volume before us we are presented with a long and minute record of cases of spinal irritation; under which name our author seems to include more diseases than are generally ranked as such by English practitioners. Thus tetanus, chorea, and many forms of paralysis, are here described. One case of traumatic tetanus recovered under the free use of opium and of tartar emetic, one grain of each having been administered every hour. Of fifteen cases of tetanus (our author does say they all were the consequence of wounds) five recovered under this plan of treatment.

Dr. Huss considers chorea to be a disease in which the spinal cord acts independently of the brain, and that the impulse in this malady is produced alike upon the extensor and upon the flexor muscles; while in tetanus the former only are affected. A kind of partial paralysis often, he says, continues long after severe attacks of either disorder. We would willingly extend to greater length our analysis of the interesting cases detailed here, but our notice of this malady will, perhaps, be already considered too long, we will, therefore, extract only one more observation relating to this portion of our subject:

> "Rheumatism," says Dr. Huss, "is but too often made the scapegoat of many obscure complaints, and of paralysis among the rest Partial paralysis may, however, occur from causes identical with those which produce rheumatism. Thus I have seen an individual who had placed his hand, at night, against a cold and damp wall, and on awaking in the morning that hand was completely paralysed. I have also treated a child which, for half an hour, had amused itself with melting ice on a frozen window with one of its fingers, and that member had become paralysed. In both cases I consider the first effect to have been produced on the cutaneous nerves, and to have been transmitted from thence to the spine; from whence, again, the paralysis was extended to the affected members."

In such cases we should, perhaps, have applied our remedies to the spine itself; but they both seem to have recovered by the endermic application of strychnia to the hands.

Diseases of the respiratory organs. Our author has already, in former reports, dwelt at great length upon these maladies; and his notice of them here is consequently short. Eight cases of diseases of the larynx and trachea were treated in the course of the year. One case occurred in a male patient, the other seven were females, and there were two deaths. The first was the cure of a girl, thirteen years of age, who had suffered from gradually increasing loss of voice and dyspnea for above a year, when suddenly, after exposure to a cold north-east wind, she was seized with acute laryngitis, which proved fatal, in spite of tracheotomy and all other remedies. On dissection, a mulberry-shaped excrescence was found occupying the ventriculus Morgagni of the left side, while the whole mucous surface of the trachea, and even down to the minute divisions of the bronchi, was closed with a firm, false membrane, about half an inch in thickness. It is curious that the patient, some years before, had had a similar excrescence on the face, which entirely disappeared, according to the report of the child's parents, on the application of a popular Swedish remedy, which was once common, too, in England, viz. the hand of a corpse.

Pneumonia. The report of this year presents us with a brief summary of 140 cases, having a mortality of 10 per cent. Of these, 23 were females and 117 males, and by far the greater number were between 20 and 40 years of age. Inflammation of the lungs is surely more frequent in these northern regions than in Great Britain. The months of April and May have for many years afforded in Sweden more than one-third of the cases of pneumonia, and the mortality was highest in June and July, which Dr. Huss ascribes to the great frequency of various abdominal complications in the early part of the summer. The relative seat of the inflammation was as follows:

Pneumonia of the right lung in 87 cases
" left lung in 39 cases
" both lungs in 14 cases.

A great disproportion was observed in the mortality of the two sexes, the deaths among the males averaging 1 in 14, and among the females 1 in 4. Pneumonia in the left side is considered by Dr. Huss as much less fatal than pneumonia of the right, the proportions being 1 death in 19 in the former case, and 1 in 10 of the latter form. Dr. Huss acknowledges that he can assign no reason for this difference. His general experience has taught him that females are more liable to double pneumonia, which is always the most fatal.

The treatment of *tubercular phthisis* appears to be in Sweden, as elsewhere, an ever disputed point. Dr. Huss fancies that he has lately obtained more benefit from moxas applied below the clavicles, than from any other mode of counter-irritation. For his general opinions regarding this malady we are referred to the Report of the preceding year.

Diseases of the circulating system. In the space of two years, 28 cases of acute pericarditis have occurred in the hospital, and of these not less than 23 were females. Unfortunately Dr. Huss again refers us to the Reports of preceding years for the details of his cases, and the mode of treatment employed. Under the head of phlebitis there is given here a well-observed instance of slow destruction of life by this malady after childbirth. Acute symptoms did not appear until the fourth week, and the patient lingered till the thirteenth week after delivery.

Dr. Huss allows of at least two causes of phlegmasia alba dolens: the one is the compression of the hypogastric and common iliac veins by the head of the infant, the other arises from pus circulating in the system. The former usually comes on soon after delivery; the latter often does not appear till the second or third week.

Diseases of the digestive organs. Simple ulcerations of the stomach are treated by our author by rest, light farinaceous diet, and the salts of silver, copper, or zinc, while counter-irritation is applied externally. He confesses, however, that the diagnosis of this malady during life is extremely difficult, and the great obscurity of the symptoms will cast a considerable shade of doubt over every recorded case of cure.

Typhoid fever. Dr. Huss considers the name of *enteritis folliculosa* as by no means universally applicable to the disease in question. He adds his testimony to that of almost all medical writers in this country, that typhus fever is most rife and severe where noxious exhalations from sewers, cesspools, &c., especially abound. During wet winters it has been remarked in Sweden that typhus was very common; but as soon as the long and steady frosts of these northern regions commenced, there ensued a great and striking abatement of the disease. We fear that travellers have been deceived by the beautiful islets and clear waters of the Swedish capital. "There is no capital in the world, at least, we think, in Europe, where so much refuse is allowed to lie in the open streets, and, above all, in the large open courts of the dwelling-houses (gardar)."

The number of typhus-fever patients in 1842 was nearly double that of

1841, in consequence of an epidemic having broken out at an early period of the year, and which supplied patients to the hospital at the rate of 50 cases per month. The total number of cases was 517, of which 342 were males and 175 females. The mortality was 10 per cent., i. e. 1 in 12 of the males, and 1 in 8 of the females, and, as usual, it was highest in those of advanced age. Of the whole 517, a preponderance of cerebral symptoms was observed in 136 cases; in 208, abdominal symptoms prevailed; and in 173 patients the signs of both affections were combined. The mortality was much the highest among those who exhibited prominent cerebral symptoms, and lowest where the two affections appeared together. Death occurred, too, at a much earlier period in cerebral cases.

The following is an abstract of an analysis of various symptoms of typhoid fever:

a. Dr. Huss insists much upon the withered loose condition of the heart in all cases of death from typhus, the muscular substance of that organ having entirely lost its firmness and elasticity. He suggests that to this loss of power of the heart is to be attributed the pulmonary congestion so constantly observed in typhoid fever.

b. Disintegration of the blood was observed in 47 cases out of 54 that were examined with this view. Dr. Huss remarks that, when death occurs after the third week, the blood is often found to have regained a certain portion of its fibrine, perhaps in consequence of the numerous secondary inflammations that are then liable to occur.

c. The spleen also, which, in typhoid fever, is generally found softened and enlarged, changes, in protracted cases, so as to regain its natural size, though its texture becomes more firm and granular than in the normal state. A softened and enlarged spleen was most frequent in those who died with predominant symptoms of cerebral disturbance.

d. The peculiar typhoid ulcerations of the intestinal canal were found in 39 cases out of 54, which is certainly a larger proportion than generally occurs in England. In 1841 the proportion in Stockholm was as 6 to 1; this year it was 3½ to 1. Dr. Huss adheres still to the opinion he expressed in former Reports, that in proportion as the typhoid exanthema shows itself more on the external surface of the body, so is it diminished in the intestines.

At page 151 we find an interesting account of the epidemic typhus fever which prevailed in December, 1841, and January, 1842, in the police barracks (gendarmerie) at Stockholm. Of 280 men constituting this portion of the force, 64 were affected with typhus fever in the space of six weeks. Thirteen cases illustrating the three classes of typhoid fever before mentioned are here given at great length, and they are followed by an extended analysis of the symptoms of the disease. The cases of most frequent occurrence in this epidemic were of the mixed class, or those wherein the cerebral and abdominal symptoms were of equal intensity, and Dr. Huss here repeats his firm conviction, that all the three classes or varieties of typhoid fever here named are alike the results of a similar morbid process, and are therefore to be considered as one and the same disease.

a. Cerebral form (page 167). Dr. Huss divides his history of the symptoms into two periods. In the first, along with the usual indications of cerebral typhoid fever, hemorrhage from the nose frequently occurred, but it does not seem to have exercised any peculiar influence on the duration or character of the malady.

A species of typhoid eruption was observed in all the cases, appearing as light red streaks (not the *tâches rosées lenticulaires*) over the chest and shoulders, and spreading from thence over the whole body, but rarely extending to the face. These streaks exhibited a tendency to assume a deeper hue, and at length, in the severest cases, they became of a deep violet, and the intensity of their colour was in exact proportion to the severity of the symptoms.

b. Abdominal typhus. In this form the usual well-known symptoms appeared, except that we do not find any distinct mention of the roseate lenticular spots so common in this type of fever. The typhoid exanthema, (?) however, before spoken of, was here again observed, but it never assumed the deep blue or violet colour. In four cases there were, however, spots like those of purpura, which *did not* disappear on pressure. When much diarrhea had occurred, there was generally excessive emaciation, and the recovery was exceedingly slow.

Dr. Huss was often able to foretell the approach of convalescence from the fact of the patient's assuming a posture different from that which they had kept throughout the severity of the disease, which had been generally the recumbent position on the back.

After a minute historical analysis of these three types of typhoid fever, Dr. Huss proceeds to consider the various peculiarities of this epidemic.

1. *Varieties.* Under the first head he acknowledges that there was often considerable difficulty in determining the exact species of fever, as at one time abdominal, and at another cerebral symptoms predominated in the same case. Sometimes, too, abortive cases occurred, where the fever came on severely at first, but disappeared suddenly upon an emetic or purgative being given, and without running its usual course. All epidemics present numerous instances of this kind, but it is still uncertain whether these cases would have become really severe if they had been left entirely to nature.

2. *Complications* occurred but rarely, and were chiefly confined to affections of the respiratory organs. It is remarkable that those of the gendarmerie who had previously exhibited symptoms of phthisis were peculiarly free from the influence of the epidemic.

3. *Sequelæ.* In two cases where the cerebral symptoms had greatly predominated, almost total deafness ensued, and continued for a long period. Œdema of the legs and feet was very frequent where the heart's action was so much weakened, that the first sound was scarcely audible, but it yielded rapidly to tonic treatment.

4. *Terminations and prognosis.* Critical symptoms, especially prolonged sleep and copious perspiration, occurred in twenty-nine cases, or in nearly one half of those affected, and Dr. Huss considers the tendency to crises to have been here much greater than in ordinary typhoid fever.

5. *Causes.* Two thirds of the cases occurred in individuals under 30 years of age. We have, in this particular instance, peculiar advantages for ascertaining the influence of age, temperament, &c., in exciting disease, as all the individuals affected were submitted to the same conditions of life, and all, too, were equally exposed to a similar routine of duty.

One principal exciting cause of the disease is thought to have been the constant rain during the night, when the guard was patrolling, and this severe weather lasted almost without intermission for above two months. The men came in from patrol (a duty of ten to eleven hours' duration) with their clothes perfectly saturated with rain, and their moist garments were hung up to dry in the crowded room in which they slept, which was moreover raised to a high temperature to facilitate the process of desiccation. By this heat and moisture, with the exhalations from so many human beings congregated in one apartment, our author thinks that a typhoid miasm was at length generated. An immediate mitigation of the violence of the symptoms was the consequence of a change of situation, effected by removing the patients to a purer air and to better ventilated apartments, while the cases that subsequently appeared were all of a milder character. The disease did not appear to be of a contagious nature, as it did not spread in a single instance among the other patients, when the sick men were removed to the general hospital. Those who had served in the patrol for only a few weeks or months were as susceptible of the disease as the veterans, who had been for years in the force.

6. *Nature of the malady.* Dr. Huss believes this epidemic to have consisted in an alteration of the blood, both in its physical constitution and in its vital properties, occasioned by the influence of the miasm brought into contact with it in the capillaries of the lungs. A similar epidemic is stated to have occurred among the workmen employed in excavating a canal in the Royal Park at Stockholm, during the months of August and September. In this instance the works were for some time carried on in a deep morass. It is to be regretted that no chemical investigation of the blood of the individuals affected by this epidemic was attempted, for, although few results of great immediate importance have hitherto been obtained by these researches, still it is obviously the line by which great discoveries are to be made, and every additional observation will prove of value. The quantity of serum was apparently in most cases diminished, the coagulum was loose and imperfect, and sometimes did not form at all, indicating a great deficiency of fibrine. In the two fatal cases the usual symptoms of typhoid action were observed—the fluid condition of the blood, the softened spleen, the atrophied state of the heart, and the swollen appearance of the glands of Peyer and Brünner in the abdomen; from which, however, as Dr. Huss observes, we are by no means to infer that these glands were so affected in all those cases which recovered. The more robust the patient, the more intense in general were the cerebral spmptoms, while the abdominal type predominated in those of a lymphatic and less sanguine temperament.

7. *Treatment. External remedies.* Venesection was only practised at the commencement of the malady, when there was much evidence of congestion in the brain and lungs. Beyond the fourth day it was never resorted to. When the abdomen was painful on pressure, especially over the iliac regions, cupping over that part was frequently practised with success, and was occasionally repeated up to the seventh day; but not after that period. Blisters, sinapisms, and turpentine epithems, particularly the last, were found to be of great benefit as derivatives, and, above all, were useful in counteracting the frequent tendency to congestion in the

lungs and trachea. Ice, in many cases, was agreeable to the patient, as an external application; but wherever it irritated rather than soothed the sick, it was wisely discontinued.

Internal remedies. Purgatives (not of the drastic kind) were freely given in all forms, unless the diarrhea and abdominal complication was excessive; but Dr. Huss derived still greater advantages from the free use of the mineral acids, especially of the muriatic acid in decoct. altheæ, and this he continued to give as long as the pulse remained full (whether its beats were sharp or soft), and the sounds of the heart normal. The varied state of the tongue, or an increase of tenderness in the bowels, formed no contraindication to its use; but it could not be employed where a tendency was exhibited to congestion of the lungs or bronchi. When the pulse became small, and the first sound of the heart weak and short, so as nearly to resemble the second sound of that organ, the mineral acids were no longer available; and Dr. Huss had then recourse to phosphoric acid, which he combined with infusion of ipecacuan, where diarrhea indicated the presence of abdominal ulceration. Camphor was found to be very valuable in all those cases where the shortening of the first sound of the heart, and the diminution of its intensity, indicated debility of that organ. Dr. Huss considers this symptom, when it occurs, to be a sure indication for the employment of camphor, *and the only real one;* for no certainty can, he thinks, be obtained in this respect from the appearance of the tongue, from the pulse, or from the nature of the delirium. Where, however, there was diarrhea, accompanied with a deep-flesh-red tongue, and also tenderness on pressure of the abdomen, he observed that the use of this drug often gave rise to vomiting and colicky pains in the abdomen. Might not the combination with hyoscyamus have obviated this inconvenience?

Musk is thought by our author to be indicated when the patient lies motionless on his back, with low, muttering delirium, plucking of the bedclothes, and subsultus tendinum; when the pulse is small and scarcely to be felt, and, above all, when the first sound of the heart is no longer to be distinguished. The doses must be large (three to five grains several times a day), or no good results can be expected.

Our author speaks favorably of opium, when the delirium is of a more violent character; but the skin must be cool and soft, and there must be no other signs of cerebral congestion. Belladonna may be employed in similar cases; but it seldom produces any effect until its well-known influence on the pupils is developed. Sulphuric acid was employed with favorable results during convalescence, especially when the vital powers were much impaired; as also in the diarrhea which sometimes persisted long after the febrile symptoms had disappeared.

Dr. Huss considers the employment of the ethereal oil of turpentine to be one of the greatest improvements in the treatment of certain complications of typhoid fever. It is of essential value, he says, in the typhoid form of pneumonia, which is so apt to show itself between the eleventh and fourteenth day of the disease; particularly in those cases where the debility of the heart's action is announced by the absence or diminution of the first sound of that organ. The weakened heart is then unable to propel the blood through the capillaries of the lungs, congestion ensues, and proceeds to inflammation of a low typhoid kind. The other indications for the employment of turpentine, in this complication, are cough with viscid and more or less bloody expectoration, dulness on percussion over some portion of the chest, particularly at the back part, with crepitation and tubar, or bronchial respiration there. Dr. Huss gives the following formula as a convenient mode of administering this medicine: ℞. Ætherol. terebinth. ʒss; vitell. ovor. No. j; mucilag. gumm. Arab. ℥iij; Aq. destillat. ʒj; M. ft. emulsio. signr. One teaspoonful to be given every hour in the daytime, and one grain of opium, in two doses, during the night.

8. *Duration.* The average of the whole cases gives twenty-three days as the mean duration of each case.

We regret that we cannot devote more space to the remaining contents of this volume, which chiefly relate to various diseases of the digestive organs. A curious instance is given (along with a well-executed engraving) of adipose tumours occupying a rather unusual seat, viz. the inner surface of the small intestine. These tumours, about twelve in number, hung by a small pedicle from the internal surface of the ileum. They were composed of adipose cells, inclosed in a common cyst, and covered by the mucous lining of the intestine. Their origin, therefore, was probably in the submucous cellular tissue.

The Report of this year is very brief in regard to diseases of the liver. An interesting history is detailed at great length of a man who, during life, had suffered from that rare form of hemorrhoidal tumour, the erectile (described by Earle and Copeland), and in whom, after death, the liver was found studded with tumours of a similar character. Dr. Huss considers these latter to be fully entitled to the much-disputed appellation of hemorroids of the liver ("hæmorrhoider pa lefvern"); but it seems doubtful whether the original tumours in the rectum can fairly claim that title.

Our author is, we think, somewhat imbued with the doctrines of Schönlein regarding the influence of the portal system in the production of disease. In one case, which he relates, there was found cirrhosis of the liver to a high degree, while the vena porta was much dilated; and on its inner surface there were observed numerous bony cicatrices, similar to those so often seen in the dilated aorta, while the veins opening into the vena porta presented the like appearance. These alterations Dr. Huss does not allow to be the result of primitive inflammation in these bloodvessels; but he maintains that the cirrhosis of the liver was the primary disease, which, by obstructing the free flow of blood through the vena porta, caused dilatation of that blood-vessel; and this, when long continued, produced a chronic inflammation, with thickening and deposition of osseous matter.

We trust that this brief analysis of Dr. Huss's Report will not have been uninteresting to our readers, and that, in common with ourselves, they will wish for some further acquaintance with this author's writings. A patient spirit of investigation characterizes the whole volume; and while Dr. Huss, in this respect, may take his stand with the most diligent pathologists of Germany, he is far superior to the brilliant writers of France in his intimate acquaintance with the medical literature of other countries.

Art. XII.

Mesmerism in India, and its Practical Application in Surgery and Medicine. By James Esdaile, m. d.—*London*, 1846. 12mo, pp. 287.

In our article on Mesmerism, eighteen months ago, we entered into an extended discussion concerning the validity of mesmeric facts, and the theory of *animal magnetism;* avoiding, however, all detailed examination of mesmeric doctrines in their application to the treatment of disease. We passed lightly over this latter topic, because, in dealing with the subject as a matter of evidence affecting its reality, we had consumed the whole of our space at that time available; yet we expressed a hope that a future opportunity would arise for supplying the omission in question. This opportunity, the appearance of Dr. Esdaile's book now presents to us.

Dr. Esdaile occupies the post of civil assistant surgeon to one of the hospitals in Bengal; a situation which furnishes him with ample means for testing the value of controverted modes of treatment. The experience which he has given to the world in this book is, assuredly, not a little startling: it has relation to the practical application of mesmerism in both surgery and medicine. The statements which he has put forth, we shall summarily notice; and then we shall submit a few remarks upon their credibility, and upon the consequence which, we think, should flow from them.

Our author informs us that, prior to his own experiments, he was almost altogether unacquainted with the subject; having neither read mesmeric books, nor witnessed experiments in the practice of others. The first case which came beneath his notice was quite accidental; and this circumstance is cited by himself as enhancing the credibility of the accounts which he publishes. He states, that had he selected for his first essay one whom he should have deemed to be an appropriate subject, he would most probably have taken "some highly sensitive female of a nervous temperament and excitable imagination, who desired to submit to the supposed influence." He observes that in the instance in question, however, he was guided by no previously adopted theory, but was determined purely by accident in the choice of his subject, who happened to be a Hindoo felon, of the hangman caste, condemned to labour in irons on the roads. Though somewhat lengthy, we will insert the details of this experiment in Dr. Esdaile's own words; previously directing the reader's attention to certain facts upon which the author himself lays great stress, as dissipating, in his own judgment, every source of deception and fallacy; these are: 1st, the purely accidental and unpremeditated nature of the experiment; 2d, all want of consent between the parties; 3d, the operator's want of belief in his own power, and slight knowledge of the subject; 4th, the absolute ignorance of the patient, who could not possibly have heard of mesmerism; and 5th, the impossibility deduced from the foregoing considerations, of the patient's imitating the mesmeric phenomena.

"*First experiment.* Mádhab Kaurá, a hog-dealer, condemned to seven years' imprisonment, with labour on the roads, in irons, for wounding a man so as to endanger his life, has got a double hydrocele. He was ordered to be taken from the jail to the charity hospital, to be operated upon.

"April 4th. The water was drawn off one side of the scrotum, and two drachms of the usual cor. sub. injection were thrown in. On feeling the pain from the injection he threw his head over the back of the chair, and pressed his hands along the course of the spermatic cords, closing his eyelids firmly, and making the grimaces of a man in pain. Seeing him suffering in this way, I turned to the native sub-assistant surgeon, an *élève* of the medical college, and asked him if he had ever seen mesmerism. He said that he had seen it tried at the medical college, but without effect. Upon which I remarked, 'I have a great mind to try it on this man; but as I never saw it practised, and know it only from reading, I shall probably not succeed.' The man continued in the position described, I placed his knees between mine, and began to pass my hands slowly over his face, at the distance of an inch, and carried them down to the pit of his stomach. This was continued for half an hour before he was spoken to, and when questioned at the end of this time his answers were quite sensible and coherent.

"He was ordered to remain quiet, and the passes were continued for a quarter of an hour longer—still no sensible effect. Being now tired (thermometer 85°), I gave it up in despair, and declared it to be a failure. While I rested myself the man remained quiet, and made fewer grimaces, and when ordered to open his eyes he said there was smoke in the room. This roused my attention, and tempted me to persevere. I now breathed on his head, and carried my hands from the back of his head over his face and down to the epigastrium, where I pressed them united. The first time this was done he took his hands off his groins, and pressed them both firmly down upon mine, drew a long breath, and said 'I was his father and mother, and had given him life again.' The same process was persevered in, and in about an hour he began to gape, said he must sleep, that his senses were gone; and his replies became incoherent. He opened his eyes when ordered, but said he only saw smoke, and could distinguish no one; his eyes were quite lustreless, and the lids were opened heavily. All appearance of pain now disappeared; his hands were crossed on his breast, instead of being pressed on the groins, and his countenance showed the most perfect repose. He now took no notice of our questions; and I called loudly on him by name without attracting any notice.

"I now pinched him without disturbing him, and then asking for a pin in English, I desired my assistant to watch him narrowly, and drove it into the small of his back. It produced no effect whatever; and my assistant repeated it at intervals in different places as uselessly. His back had continued to arch more backwards latterly, and he was now in a state of 'opisthotonos;' the nape of his neck resting on the sharp back of the chair, and his breech on the edge of it. Being now satisfied that we had got something extraordinary, I went over to the Kutcherry, and begged Mr. Russell, the judge, and Mr. Money, the collector, to come and see what had been done, as I wanted the presence of intelligent witnesses in what remained to do. We found him in the position I had left him in, and no hallooing in his ears could attract his attention. Fire was then applied to his knee, without his shrinking in the least; and liquor ammoniæ, that brought tears into our eyes in a moment, was inhaled for some minutes without causing an eyelid to quiver. This seemed to have revived him a little, as he moved his head shortly afterwards, and I asked him if he wanted to drink; he only gaped in reply, and I took the opportunity to give, slowly, a mixture of ammonia so strong that I could not bear to taste it; this he drank like milk, and gaped for more. As the 'experimentum crucis,' I lifted his head, and placed his face, which was directed to the ceiling all this time, in front of a full light; opened his eyes, one after the other, *but without producing any effect upon the iris*; his eyes were exactly like an amaurotic person's, and all noticed their lack-lustre appearance. We were all now convinced that total insensibility of all the senses existed, and I ordered him to be placed on a mattress on the floor, and not to be disturbed till I returned. It was now one o'clock, the process having commenced at 11 a.m.

"I returned at three o'clock, and was vexed to find that he had awoke, and been carried back to the jail hospital. The native doctor of the jail had come in, and on hearing that the Sahibs could not awake the patient, he set about doing so, and succeeded by throwing water on his face."

Dr. Esdaile maintains that an especial value attaches to early experiments in any new subject of investigation. In reference to his own trials of mesmerism in the first instance, he likens his position to that of the chemist who, having heard that a new elementary substance had been discovered by a certain process, sets his own apparatus to work in the way prescribed, and is rewarded by obtaining the same results as the first discoverer; these, Dr. Esdaile observes, surprise the experimenter even, who feels confident that he only relates what he actually saw, and that he is not seduced by previous theory and prepossession of mind to interpret appearances in support of a foregone conclusion.

Having, in the manner just stated, become satisfied that there was a truth in mesmerism, Dr. Esdaile informs us that he lost no time in turning it to practical account: and the success which he recounts surpasses all previous records of this nature. If we may credit the publication now before us, it would appear that almost every attempt to reduce a patient to the mesmeric condition was more or less successful. Indeed, the statements which are made regarding the facility of inducing insensibility prior to surgical operations are, to say the least, very remarkable, we may say, not a little suspicious. Within the period of eight months, viz. from May 1845 to January of the present year, Dr. Esdaile reports himself to have performed no less than seventy-three painless operations; and some of these were of the most formidable character, as may be inferred from the following synopsis furnished in a prefatory Report of Mesmeric Facts:

> "*A return showing the number of painless surgical operations performed at Hooghly during the last eight months.* Arm amputated, 1. Breast ditto, 1. Tumour extracted from the upper jaw, 1. Schirrus testium extirpated, 2. Penis amputated, 2. Contracted knees straightened, 3. Ditto arms, 3. Operations for cataract, 3. Large tumour in the groin cut off, 1. Operations for hydrocele, 7. Ditto dropsy, 2. Actual cautery applied to a sore, 1. Muriatic acid ditto, 2. Unhealthy sores pared down, 7. Abscesses opened, 5. Sinus, six inches long, laid open, 1. Heel flayed, 1. End of thumb cut off, 1. Teeth extracted, 3. Gum cut away, 1. Prepuce cut off, 3. Piles ditto, 1. Great toe nails cut out by the roots, 5. Seton introduced from ankle to knee, 1. Large tumour on leg removed, 1. Scrotal tumours, weighing from 8 lb. to 80 lb., removed 17, painless, 14.—Operations, 73."

This statement, we conceive, involves an amount of success in the production of the mesmeric coma, far beyond any thing that is recorded in this country, or indeed in Europe. Dr. Esdaile assigns a purely hypothetical, we may say visionary cause for this. He attributes its comparative rarity in Europe to the "influence not being at once sufficiently concentrated on the patient, by transmitting it to his brain from all the organs of the operator, and through every channel by which it can be communicated." He says that, in India, with due patience and sustained attention, coma, in a tolerably extensive field of experience, can be produced daily, so that insensibility to the surgeon's knife will ensue; the process adopted by himself is set forth in the subjoined extract:

"Desire the patient to lie down and compose himself to sleep, taking care, if you wish to operate, that he does not know your intention: this object may be gained by saying it is only a trial; for fear and expectation are destructive to the physical impression required. Bring the crown of the patient's head to the end of the bed, and seat yourself so as to be able to bring your face into contact with his, and extend your hands to the pit of the stomach, when it is wished; make the room dark, enjoin quiet, and then shutting your patient's eyes, begin to pass both your hands, in the shape of claws, slowly, within an inch of the surface, from the back of the head to the pit of the stomach; dwelling for several minutes over the eyes, nose, and mouth, and then passing down each side of the neck, go downwards to the pit of the stomach, keeping your hands suspended there for some time. Repeat this process steadily for a quarter of an hour, breathing gently on the head and eyes all the time. The longitudinal passes may then be advantageously terminated, by placing both hands gently, but firmly, on the pit of the stomach and sides; the perspiration and saliva seem also to aid the effect on the system.

"It is better not to test the patient's condition by speaking to him, but by gently trying if the cataleptic tendency exists in the arms. If the arms remain fixed in any position they are left in, and require some force to remove them out of every new position, the process has been successful; the patient may soon after be called upon by name, and pricked, and if he does not awake the operation may be proceeded with."

It would carry us beyond the limited space which we can give to the present notice, were we to furnish any detailed account of the various operations performed by Dr. Esdaile; it is not, however, necessary to do so. It has been seen in what way he was led to experiment in the first instance, and the particulars of the subsequent cases, in which the knife was employed without eliciting signs of sensibility, do not differ from the generality of this class of facts, exemplified in our former article upon this subject. We may, in the sequel however, have occasion to refer to some particulars of individual cases, in briefly discussing the credibility of these narratives.

We will now refer to the statements which are made concerning the alleged therapeutical action of mesmerism. Dr. Esdaile says, that it is useful in chronic inflammation, by gently stimulating the nerves and capillary vessels to more healthy action; and, with respect to functional derangements of the nervous system, he hopes that, at length, in this mesmeric agency, we have obtained a *direct* nervous remedy. The medical cases adapted for the use of mesmerism did not very often occur in our author's practice; because, says he, "the labouring poor do not usually resort to medical advice for nervous diseases till they are past cure;" he yet furnishes accounts of a number of them which he considers to have been beneficially influenced by the new remedy. The particulars correspond very much with the accounts which are given by other writers on mesmeric therapeutics. They are, on the whole, very unsatisfactory, like their predecessors. Not a few of them leave ample room for doubt, first, as to the reality of the disease treated; and, secondly, admitting their reality, as to the cure being effected by the alleged means.

The following is a summary of the medical cases said to have been treated successfully by mesmerism, within the before-mentioned period of eight months:

"*A return of medical cases cured by mesmerism during the last eight months*

Nervous headache, 3 cured by one trance. Tic-doloureux, 1 ditto. Nervousness, and lameness from rheumatism of 2½ years' standing, 1 by chronic treatment. Spasmodic colic, 1 by one trance. Acute inflammation of the eye, 1 by repeated trances in 24 hours. Chronic ditto, 1 by chronic treatment. Acute inflammation of testes, 1 by repeated trances in 36 hours. Convulsions, 1 by one trance. Lameness from rheumatism, 2 by chronic treatment. Lumbago, 1 by general and local mesmerising for a week. Sciatica, 1 ditto. Pain in crural nerve, 1 ditto. Palsy of one arm, 1 ditto for a month. Ditto of half the body, 1 ditto for 6 weeks. Feeling of insects crawling over the body, 1 by one trance.—18."

Now we maintain that we are not entitled, in reason, to reject the facts, or alleged facts above stated without at least a fair examination. It were very easy for us to chime in with the ordinary professional ridicule, in relation to such statements; but, in common honesty, we conceive that we are bound to take a different course. We think it incumbent upon us, fairly to investigate their consistency and authenticity, and to determine the respect or the derision with which they are to be treated, according to their probable truth or falsehood. For the present we shall confine our attention exclusively to the surgical cases; and the first question we have to discuss is this: *Are the statements* FACTS?

In our former article we adduced reasonable evidence to show that cases of coma, unattended by serious or permanent lesion, did sometimes arise spontaneously as disease; that, in such cases, all indications of sensibility were very often absent; and that, as respects numerous classes of mesmeric cases, the only novelty which attaches to them consists in their artificial production. Whether coma and insensibility can or cannot be induced by the mesmeric process is a matter to be decided by experience only; if we have not ourselves this experience, but rest satisfied with estimating the value of that which is recorded by others, we must, in the formation of a judgment, be guided by the common rules of evidence. In this state of the question we reiterate our former position, that sufficient grounds do actually exist for admitting both the possibility and the probability of coma being artificially produced. The extent to which the statements of particular individuals may justly be credited is, of course, a very different question. But when an honest and intelligent witness circumstantially relates, as having occurred, what is already admitted to be neither impossible nor improbable, the presumption, *à priori*, is certainly altogether in his favour.

In the volume before us we have a well-informed and intelligent surgeon telling us, that within a period of eight months, no less than 73 surgical operations were performed by himself, without there being any indications of suffering on the part of the patients; the whole of the circumstances, moreover, to which reference is made, being abundantly authenticated by respectable names. To suppose the existence of wilful misstatement, on the part of Dr. Esdaile, is out of all question, where detection of falsehood would follow so surely; for our own part we feel confident that, in the instances adduced, there exists, at the very lowest estimate, a *basis* of fact. That our author and his associates should have conspired to delude, and to mystify their friends and their countrymen at home, without any conceivable motive, we hold, indeed, to be too ridiculous to imagine for a moment; and we believe it to be almost as unreasonable to suppose that, in so large a number of instances as those related, individuals should uniformly, as if by common consent, have repressed the very strongest instincts of our nature in avoiding every expression of feeling under agonizing torture,—and that, too, for the mere purpose of deceiving the bystanders. If, however, we could escape the difficulty by admission of this latter explanation, how must we dispose of the first experiments? The earlier patients, who were exclusively of the lowest and most ignorant castes, could certainly know nothing of mesmeric indications; least of all so completely, as successfully to simulate them. On every sound canon of evidence, then, we must admit the existence of *some* reality in Dr. Esdaile's facts. Let us remember that the statements involve nothing that we should deem to be impossible, even as natural occurrences, for physiological states, analogous to those now under discussion, have arisen spontaneously; besides well-authenticated instances of insensibility to external impressions have been developed here, in our country, by artificial means; we ourselves can, to a limited extent, testify to this circumstance, as affirmed in our former article. Why, then, should we hesitate to receive Dr. Esdaile's statements with calm respect, and with a disposition to give to them an unprejudiced investigation? It is certainly true that, out of the great number of alleged facts, there are many which are by no means proved to be facts, and not a few which were, in our mind, most probably sheer imposture; we yet reiterate our own conviction that it is most unreasonable to doubt that *some* were *bona fide* cases of artificially induced insensibility. If much of what is here averred were not so very unexpected and wonderful, so much opposed to common notions and every-day experience, and—shall we say?—to our prejudices, it is quite sure that we should never dream of calling it in question.

But although, on many accounts, ready to admit a reality in the reported experience of Dr. Esdaile, as well as in that of a corresponding character nearer home, we would yet caution the reader against the *too facile* reception of this class of facts; to the *proof* of which, undoubtedly, the evidence very often is far from being adequate. It is too common for witnesses in this matter, medical not less than lay, in the first instance to disbelieve altogether in the possibility of inducing, in any degree, states called mesmeric, and then, once satisfied of the contrary, to place almost implicit credence in every statement relating to the subject, however improbable, and however imperfectly attested. As past experience has exhibited cases of temporary insensibility to external impressions, irrespective of mesmerism, and as this fact justly disposes us the more readily to credit the reality of such states, when said to be developed under other circumstances, so, in like manner, past experience has repeatedly displayed instances where the most heroic endurance of pain has enabled the sufferer to repress the outward manifestations of it, and at times, too, for no other purpose apparently than the pure love of deception—a disposition very well known to attack some minds as disease. In judging of the fact of alleged insensibility in particular instances, consideration should of course be always directed to this circumstance. We rarely, however, read mesmeric statements, or see mesmeric experiments, without being struck with the fact of how little evidence, ordinarily, satisfies the all-believing professed mesmerist.

Dr. Esdaile, in the work we have under review, supplies, we think, illustration in some degree of what we have just advanced. He does not appear to us to have been sufficiently aware of the *possibility*, or even the *probability*, of deception,—a circumstance most likely attributable to his having, at the very onset of his career, adopted the general views and the speculations of the animal magnetisers. It is true he states that, prior to his own experiments, his mind was all but a blank to the subject; that he had never seen mesmerism as practised by others; and that he had not even read a mesmeric *book;* but then he acknowledges himself to have read "scraps of newspapers" (most unphilosophical sources of a first impression); he states that he had paid attention to all well-attested reports upon the subject, and that he had ceased to regard it lightly, "ever since Dr. Elliotson declared, years ago, that he should despise himself if he did not declare his conviction of the truth of mesmerism." We do but refer to all this as showing that Dr. Esdaile's mind was *not* so divested of prepossession as he himself appears to have imagined; this consideration does not in any way invalidate the statements, but it constitutes a reason for the exercise of caution in determining the *degree* of their credibility. Moreover, there is another circumstance to which we would direct attention: the patients, very generally, were natives of India, and of the lowest and most degraded castes, and sometimes felons. Now it is, we believe, pretty notorious that the Hindoo race is characterised, in a very marked manner, by cunning and deception, and that they have especial satisfaction in deceiving the European; and we should naturally expect these unfavorable traits of natural character to be prominently displayed in the most ignorant and the least moral portion of the community. Neither can we forget the astounding constancy and self-possession with which this race of men endure self-inflicted tortures of the most agonising kind in the exercise of their religious superstitions. Dr. Esdaile does not appear to us to have sufficiently thought of these matters; yet he testifies to the accuracy of the Hindoo characteristic just referred to, where, in speaking of the proceedings of the first French commission of inquiry, he compares D'Eslon to a *Bengal witness*, whom he describes as "not content to tell the truth simply, but added so many corroborating inventions of his own, that no one knew what to believe."

In confutation of the idea that he himself could have been imposed upon by the patients upon whom he had operated, our author remarks as follows:—

> "Since then I have had, *every month*, more operations of this kind than take place in the native hospital in Calcutta in a year, and more than I had for the six years previous. There must be some reason for this, and I only see two ways of accounting for it: my patients, on returning home, either say to their friends similarly afflicted, 'Wah! brother, what a soft man the doctor Sahib is! He cut me to pieces for twenty minutes, and I made him believe that I did not feel it. Isn't it a capital joke? Do go and play him the same trick; you have only to laugh in your elbow, and you will not feel the pain.' Or they say to their brother sufferers, 'Look at me; I have got rid of my burthen (of 20, 30, 40, 50, 60, or 80 lbs., as it may be), am restored to the use of my body, and can again work for my bread; this, I assure you, the doctor Sahib did when I was asleep, and I knew nothing about it. You will be equally lucky, I dare say; and I advise you to go and try; you need not be cut if you feel it.' Which of these hypotheses best explains the fact my readers will decide for themselves."

This quotation, we allow, constitutes a very fair method of dealing with the objections of those who would maintain the whole affair to be *humbug*, and that a state of coma and insensibility had in no case been provoked artificially; yet it is very easy to conceive the existence of not very extraordinary *motive* for individuals to affect and to simulate such a condition, *after* the rumours had gone abroad,—in some instances justly,—regarding the painless character of Dr. Esdaile's operations; and the very fact of such rumours having had, *occasionally*, a solid foundation sufficiently accounts for the popularity which the particular hospital obtained. Individuals, from determination not to be outdone, or even from pure love of deceiving others *might* sham. We do not say that this was actually the case; but the possibility—nay, probability—of the thing suggests itself to us on perusal of the book; and we think that the idea is entitled to attention in forming an ultimate estimate of the statements.

The following case, to our minds, is most unsatisfactory; we insert the account as exemplifying what we deem to be the obvious disposition in our author to be satisfied of the genuine character of his facts upon extremely slender evidence:

> "August 13th. Dr. Finch freely applied muriatic acid (such as is furnished by the Company's dispensary) to a sore, covering all the right temple of the woman Gendo (who was mesmerised, in his presence, by one of my assistants) without her showing the smallest degree of consciousness; and it was with great difficulty I awoke her, after he had failed to do so. During the burning with the acid her pulse fell from 88° to 80°, and *her respiration, which was quite natural before she was mesmerised, became entirely diaphragmatic and abdominal; the voluntary and semi-voluntary muscles of the chest being completely paralysed.*
>
> "August 18th. Dr. Bedford, to-day, asked permission to apply the acid to the woman Gendo's sore when she was awake; and though I thought this most irrational scepticism (he having witnessed Dr. Finch's experiment) I consented, in order that it might not be said I interfered to save my phenomena. He accordingly wetted the end of the glass stopper with the acid, and touched the sore with it, and *the woman, for a few seconds, did not show any signs of acute pain.* There could be no doubt about it, she was found out! The arch-deceiver, having set a snare and delusion for me, was now laughing at my beard; and I was not relieved from my thick-coming fancies by Dr. Bedford kindly suggesting 'that she was probably a very insensible person naturally.' I was soon roused from my trance of bewilderment, however, by hearing the woman cry out that 'we had put pepper to her head,' and she sat up, showing signs of great pain; immediately after she declared 'her head was on fire,' and got out of bed, walking about distractedly in great agony. I ordered her head to be bathed, and, as the best anodyne, threw her into the trance. The sore being surrounded by tubercles, which retarded its healing, I took the opportunity to pare them off; and to this she was perfectly indifferent. In half an hour I awoke her with much difficulty, in order that Dr. Bedford might hear her first words, which were, that she knew nothing about what we were talking of, having even forgotten the burning."

The change in the character of the respiration, described as occurring "during the burning with the acid" by Dr. Finch, is precisely such as would take place under an effort to conceal the outward signs of suffering. Why, on the acid being applied in the waking state by Dr. Bedford, did not the patient, for some seconds, exhibit indications of pain? Did the manifestations of acute sensibility, after a brief lapse of time, become displayed by the patient, not instinctively, but designedly, just when she discovered that the exhibition was anticipated? Why, moreover, did Gendo retain no recollection of the occurrence after mesmerisation. There was no mesmerism at the time. Was it that the woman was mistaken, in supposing that oblivion in this point of view was expected by the doctors? We refer to these circumstances, because they illustrate the feeble points in not a few of Dr. Esdaile's narratives. Still, we would maintain that, judging of the aggregate of the statements by all the accredited rules of evidence, it is impossible to dispose of this matter of painless operations by the assumption of delusion or imposture in every case.

With respect to the accuracy of the facts relating to the efficacy of mesmerism in the treatment of medical cases, we experience a much greater difficulty in forming a satisfactory decision than we do in the case of the surgical operations; the sources of fallacy, under the former circumstances, being so very much more considerable. From the previous summary given of Dr. Esdaile's recorded experience, it will be perceived that, as is nearly always the case where unwonted methods of cure are said to be employed, diseases of the nervous system form a very large proportion of the group; a class of cases, in which the powerful agency of excited imagination and hope of improvement, howsoever induced, is so universally recognized. Nevertheless, satisfied as we are that abnormal conditions of the system do at times ensue upon the practice of mesmerism, we are quite ready to believe that it may possibly be serviceable in certain nervous affections by exciting a change of action. It is quite certain that, by employment of the "passes" and other means for continuously sustaining uniformity of impression, sleep—common sleep—can at times be procured, when narcotic drugs would either fail or lead to ulterior mischief; and, under such circumstances, it is not difficult to suppose that actual good should arise from use of the means in question. Still, in all these matters, we are ever sceptical concerning the relations of the *post* to the *propter;* for the ordinary records of mesmeric cures always remind us of the obviously mendacious or exaggerated statements attesting the efficacy of every conceivable kind of quackery that has at any time attempted to identify itself with practical medicine. Thus, there is apparently very good evidence to show that epilepsy, hysteria, and neuralgia have sometimes given way under mesmeric treatment; and it frequently seems reasonable enough to attribute the recovery to the means antecedently employed; but then we have just the same evidence to prove that almost every other disease has, at some time or another, been cured by identical processes; nay, the same testimony is adduced to exemplify the curative agency of water about which the "passes" have been made; and sometimes even the therapeutical potency of the mesmeriser's volition is set forth! What wonder if such extravagances throw us back upon our primitive incredulity?

A good many of Dr. Esdaile's mesmeric cures, even as narrated by himself, appear to us to rest absolutely upon no evidence at all, although they are yet cited by himself as undoubted instances of success. The following cases are of this kind:

> "June 6th. I was called at eight o'clock last night to see the wife of Baboo Essanchunder Ghosaul, deputy magistrate of Hooghly. I found her in dreadful convulsions; she was speechless, and suffering from a constriction in the throat, that threatened to suffocate her every minute; and she constantly beat or pointed

at the part. At one moment her body was perfectly rigid, and in another it was bent back like a bow, till she rested on the back of her head and heels only. I never saw such convulsions except in tetanus and hydrophobia, and all I knew of the resources of medicine was useless; for how could she take physic when she could not take breath? I therefore had recourse to my new solvent power, and after nearly an hour's hard work, I left her asleep and catalepsed.

"July 1st. She has had no return of the fit. This is the lady for whose relief the conjuror was sent, but came too late.

"June 26th. Alunga, aged 24: she has slight contractions of both elbow-joints, from rheumatism, with acute pain on pressing the ulnar nerve at the elbow. At first she did not bear much handling without awaking; but, on being left alone, the trance deepened, and she permitted me to work her joints like door-hinges, and extend them to the natural degree, without awaking. One arm was much freer after the first trance and extension, and there was no pain.

"June 27th. Complains of considerable pain in her left arm to-day, and the nerve at the elbow is very tender. I passed my fingers along the course of the nerve for a few minutes, which removed the pain, and allowed her to extend the arm; I then held my fingers before her eyes for a few seconds, and she fell into my arms insensible.

"July 3d. This woman's pains fly about, but I can chase them away from any part by holding my fingers over it for a short time. She came limping up to me to-day, to have the pain taken out of her 'tendo Achillis;' and this I did by passing my fingers over the pained part. I then grasped it firmly; she felt no pain, and by looks and words expressed the utmost astonishment and delight. This woman's sensibility is such, that I, or any one, can now make her delirious by merely looking at her for five minutes. But more of this hereafter.

"August 18th. I requested Dr. Bedford to satisfy himself if the woman Alunga had pain in any part of her body. On being asked, she said there was acute pain in one heel; and Dr. Bedford spent a long time in testing the reality of its existence. He at last said that he was convinced that there was considerable pain in that spot. I then passed my fingers over the part for a minute, and grasped the heel as firmly as I could, and she declared the pain had vanished; and Dr. Bedford allowed that it had. He then looked at her steadily, and in a few minutes developed the mesmeric delirium, and desire to sleep-walk, always produced in this woman, if the influence is not quickly concentrated upon her. The other symptoms, tremor of the eyelids, inability to open them when closed, and the mesmeric trance, all followed in due course." (pp. 181-4.)

In the first of the above cases we have a somewhat severe attack of hysteria; and, in one hour after the physician's arrival, the paroxysm is over, and the patient goes to sleep. There is nothing to indicate more than the natural termination of the fit; certainly, there is nothing to show that the cessation of the paroxysm could be attributed to the agency employed. But it is exceedingly difficult to deal, in reasoning, with reports of individual facts, as the unavoidable omission of many little circumstances will often interfere with the formation of an accurate judgment with respect to them.

Having, however, fully admitted the high probability of some of Dr. Esdaile's statements concerning the painless character of the surgical operations; and being, indeed, from many circumstances, well convinced that a great depression of outward sensibility, if not its temporary abolition, will, in some constitutions, result from practice of the mesmeric art, we will now proceed to the consideration of what we deem to be the reasonable corollary, from this admission on our part. We conceive, then, that the evidence attesting the fact of certain abnormal states being induced by mesmerism, is now of such a character that it can no longer be philosophically disregarded by the members of our profession, but that they are bound to meet it in the only way in which alleged facts can satisfactorily be either verified or confuted,—by observation and experiment. When it is positively affirmed that the mesmeric processes will sometimes render a patient utterly insensible to the surgeon's knife, when detailed illustrations of this fact are recorded almost every day, how can we *fairly* reject such statements, unless we go to nature, observe for ourselves, and demonstrate the *source* of the monstrous fallacy that is deluding members of the profession and the public alike? Indeed, we hesitate not to assert that the testimony is now of so varied and extensive a kind, so strong, and in a certain proportion of cases so seemingly unexceptionable, as to authorize us, nay, in honesty to *compel* us to recommend that an immediate and complete trial of the practice be made in surgical cases. If experience like that which Dr. Esdaile relates to us be but true in one tenth, nay, one hundreth of its particulars, we hold that a case is made out demanding searching inquiry. If mesmerism, even in its humbler pretensions, be absolutely untrue, let it be *proved* to be so. If careful observations and repeated experiment lead to the detection of some hitherto hidden cause of error and mistake that has deluded and mystified the more honest class of mesmerists, what a service will be rendered to humanity and to truth if this can be proclaimed on perfectly just and adequate grounds! In how much better a position shall we be *after investigation* for confuting the imposture, if such it shall turn out ultimately to be, than in continuing to treat the subject with contemptuous disregard! Of one thing let us rest assured, not only the public, but the more sober thinking of the profession will, ere long, hold those at a disadvantage, who, in opposition to *facts*, apparently well authenticated, can or will but adduce mere unsupported argument, or ridicule.

There would appear to be two conditions attaching to any novel practice in medicine, independently of the authority by which it comes recommended, that should influence its title to a fair trial; *first*, the extent of the anticipated benefit, and, *second*, the degree of possible mischief attending its employment. Now the *promised* advantages of mesmerism in surgical operations correspond with these requirements in an eminent degree. If the statements be corroborated, and if insensibility *can* be produced artificially, surely the immense acquisition both to operator and patient is obvious at once; and, according to all the evidence that exists upon this subject, mischief very rarely follows the practice of mesmerism in the event either of success or of failure. "I beg to state," says Dr. Esdaile, "that I have seen no bad consequences whatever ensue from persons being operated on in the mesmeric trance. Cases have occurred in which no pain was felt, even subsequent to the operation, and the wounds healed by the first intention; and in the rest I have seen no indication of any injurious consequences to the constitution. On the contrary, it appears to me to have been saved, and that less constitutional disturbance has followed than under ordinary circumstances." If then *good* is possibly to ensue, and *mischief* is but little to be feared from the experiment, why not candidly make it? Assuredly experiments in therapeutics are constantly made on grounds far less reasonable. If a single practitioner of any eminence recommend some novel and *heroic* treatment in serious disease, multitudes are ready to try it; however perilous to the patient the trial, *a priori*, may appear. Although, at the present day, it is pretty well made out that pneumonia, in many instances, will come to a successful issue with very little depletion, some dozen years since large numbers of the profession, especially in France, did not hesitate, on the recommendation of M. Bouillaud, to bleed *coup sur coup;* and, about twenty years ago, when Dr. Armstrong bled largely, and administered heroic doses of calomel in the incipient stage of fever, many persons felt themselves authorized in adopting the treatment experimentally. Yet, in these instances, a degree of risk to the patient was incurred in the attainment of the possible benefit, and there was, moreover, an uncertainty in deciding upon the exact nature of the result, which, as regards mesmerism in surgery, would not be experienced. Again, we say, let it be *tried* upon patients about to be submitted to the knife; if true, let us have the benefit of it, and if false let the falsehood be *demonstrated.*

Experiments relating to the treatment of medical diseases by any doubtful remedy could not result in the same decided issue, as in the example of mesmerism applied in operative surgery. We may observe, however, that when full satisfaction has been obtained respecting the efficacy of certain processes in the production of coma and insensibility, it would naturally be expected that, in various morbid states, the induction of such conditions may aid the restorative powers of the system; at any rate, in the present acknowledged paucity of positive therapeutical agents in numerous diseases, especially in those prominently affecting the nervous functions, the trial might very reasonably be made. We think sufficient evidence exists to prove the power of the mesmeric passes—or, we would rather say, the passes of mesmerists—as also of Mr. Braid's hypnotic fascination, in occasionally conciliating sleep, in nervous cases, after the failure of ordinary medical means. We may attach no importance to the *mesmerism* employed in such cases as involving specific agency; the patients may have been made to sleep very likely by the process operating as an appropriate *lullaby*, and the improvement have thus followed. It is, however, satisfactory to know that, under such circumstances, we can sometimes provoke sleep, and thereby effect a mitigation of human suffering.

We have before said that Dr. Esdaile exhibits a full and undoubting faith in most of the phenomena usually recorded as characterizing mesmerism; and thus, in reasoning upon the assumed resultant benefit in medical cases, he constantly avails himself of the theory of animal magnetism. In speaking of the curative agency, he holds it to be due to the transference of a vital fluid from the person of the mesmeriser to that of the mesmerised. We have too recently devoted a portion of our pages to this point, to reopen the discussion upon the present occasion; we can only observe that, after a careful perusal of Dr. Esdaile's book, we do not see that he has strengthened one whit the idea of exoteric animal influence—of objectivity—in mesmerism. We do not *deny* the reality of animal magnetism; we assert, however, that it is not *proved*, at least to our satisfaction.

In dealing with the subject of mesmerism, we are but desirous of coming at the truth; we have already given our reasons for thinking that the time is come when, so far as it may be false, it can only be put down by calm and unprejudiced inquiry. From the curious and extraordinary accumulation of records amassed by the animal magnetisers, we have from the beginning been very much persuaded that, at the foundation of all the extravagances of the mesmeric disquisitions, there would ultimately be discovered *some* truth. So far back as the year 1839, after presenting to the reader a brief historical account of animal magnetism, we stated that we could not " be so far influenced by the impostures occasionally practised under the name of magnetism, as wholly to deny that some of the phenomena, from time to time produced by all aspirers to the art, seem to result either from some principle heretofore unknown and not yet correctly designated, or from some modification of recognized principles in the animal economy, which cannot yet be accurately limited or defined." In our article of last year, we entered into a more careful investigation of the particular facts, attempting some discrimination of the most probably true from the almost certainly false. In the former category we included the artificially produced *sleep*, *coma*, *altered sensibility*, *spasm*, *or temporary paralysis of muscles*, *&c.*—THE SIMPLE PHENOMENA OF MESMERISM ; our admission of the reality of these conditions rested, *first*, upon their antecedent probability, as being, in most respects, analogous to abnormal states sometimes arising spontaneously as disease ; *secondly*, upon the fact of the evidence attesting them corresponding with all just requirements ; and, *lastly*, on the circumstance of having ourselves either seen them or been grossly duped in spite of a very rigid scrutiny. In like manner—exactly on the same grounds—we admitted the reality of mesmeric *somnambulism*, to the exclusion of *clairvoyance* and the other phenomena comprehended in the term *lucidity*. These latter, however, as well as the theory of animal magnetism, we only rejected as *unproved ;* because, unlike the simple mesmeric phenomena so designated, we had sought for the reality in vain, and because the evidence, when fully and fairly tested, had always appeared to us to break down—feeble and inconclusive at the best. Within the last eighteen months we have not been indifferent spectators of what has been going on with respect to these matters ; we have read, and we have ourselves investigated, with minds open to conviction and indifferent to every consideration but that of *truth*, and our conclusions remain unchanged. We must reiterate that, in our judgment, after making all due abstraction of the roguery and deceit notoriously prevalent in many mesmeric exhibitions, there is but a reality in the simple phenomena as just enumerated ; that the induction of these does not necessarily imply the existence of exoteric animal influence ; but that, as formerly maintained by M. Bertrand in France, and still more recently by Mr. Braid in this country, the mesmeric states most probably, in all cases, arise from sensible—not occult—impressions ; and that the higher phenomena, which imply the receipt of intelligence otherwise than through the customary channels, are not only in the highest degree improbable, but are utterly unsupported by the requisite force of evidence.

Art. XIII.

The Brain and its Physiology; a Critical Disquisition on the Methods of determining the Relations subsisting between the Structure and Functions of the Encephalon. By Daniel Noble, m.r.c.s.—*London*, 1846. Post 8vo, pp. 459.

> "The doctrine of the functions of the brain constitutes, most assuredly, a subject of discussion inferior to no other branch of science, either in interest, extent of its objects, or in general importance; and in physiology it must, for many reasons, be esteemed the highest department."

With this prologue to our author's introductory chapter we need scarcely express our hearty concurrence; and notwithstanding that we have on former occasions treated somewhat in full the subject of the physiology of the brain, both in its phrenological aspect and in connexion with the physiology of the nervous system in general, we feel called upon, by the peculiar position of the question at the present time, to enter again into a somewhat detailed examination of it; with the hope of throwing some new light upon it from our own resources, and of being thus able to assist in clearing the path to further discovery. Nothing can be better than the spirit with which Mr. Noble sets out upon his undertaking, as will be evident from the following extract, which follows a notice of the imperfect views current upon the subject amongst physiologists in general:

> "And yet there is no good reason why an inquiry into the functions of the brain, and of its various parts, should not be successfully prosecuted; for there is no obscurity in this department of physiological research, which is not common to many others. Cerebral physiology has its own obstacles and its own difficulties, like any other branch of science, requiring certain mental peculiarities and favorable opportunities for their removal and dissipation; but still, under just circumstances, the discovery, not only of the general office of the brain, but of the functions of its particular parts, is an object of pursuit for the philosopher as legitimate, and as likely to reward the labour of investigation, as the solution of any other physiological problem. This attempt will be found useless, or productive of imperfect or contradictory results, only so long as the method pursued is defective or faulty. All sciences, physiological not less than those purely physical, have sustained both error in their development and retardation in their progress, wherever they have been cultivated, without the guidance of sound principles in the investigation. Under such circumstances talent has but led to confusion, and industry has yielded no fruit." (p. 10.)

But what are "the sound principles of investigation" which are to guide us in this difficult department of physiology? Are they those which have been found successful in other branches of physiological inquiry, and which would naturally occur to us as our safest indicators in the exploration of a new and unexplored path? Or are we to discard these tried and faithful guides, and to rely exclusively upon others, which have been long since demonstrated to be insufficient to conduct us to a valid conclusion, in almost every other portion of the inquiry into the functions of the human structure? At this starting point, we regret to find ourselves so far from agreeing with Mr. Noble, that our further paths must be widely divergent. For the main object of his work is to prove, that the physiology of the brain of man must be first evolved by the comparison of the relative development of its different parts, in different individuals of the human race, with the proportionate manifestation of the various powers, propensities, &c., in the characters of these individuals respectively; and that the validity of the inferences derived from comparative anatomy must be tested by their coincidence with those thus deduced from observation on man. We, on the other hand, are prepared to maintain that comparative anatomy is here, as elsewhere, our most trustworthy guide; and that the validity of any inferences drawn from observation of the coincidence between the development and the functional manifestations of the various parts of the human encephalon, must be tested by their conformity with its obvious and decided indications.

We must do Mr. Noble the justice to say that he makes out what will doubtless appear to a large proportion of his readers a most excellent case. He hits away, right and left, at comparative anatomists and vivisectors, and at physiologists who pin their faith upon them; and demolishes all their assertions and arguments with such cleverness, and so completely to his own satisfaction, that none but those who have previously made themselves well acquainted with the subject are likely to do otherwise than accord with him. His confidence in the fundamental correctness of the method of Gall is so great, that he unhesitatingly undertakes to defend it against all comers; his admission of possible improvements in it being so limited as not at all to affect the ardour in which he upholds it, as affording the only sound exposition of the functions of the encephalon.

But he does not give us any sufficient explanation why, in this particular case, we are to abandon that philosophical method of studying physiology upon the basis of comparative anatomy; the advantages of which are every day becoming more apparent. His arguments on this point may be reduced to these two;—the supposed difficulty of determining the real analogies of the several parts of the nervous system, in animals constructed upon different types;—and the want of materials for a reasonable psychology of the lower tribes of animals. Now in regard to the first point, we do not hesitate to say that the difficulty is more apparent than real. Mr. Noble has made the most of it, by pointing out the discordance on this subject in the writings of comparative anatomists; but this discordance was much greater in the infancy of the science than it is now, and it is gradually giving way to a very satisfactory harmony of opinion, as the principles upon which the investigation must be conducted are being gradually evolved. Such discrepancies are common to the early stage of all sciences. It would be equally to the purpose to quote the early mistakes of Gall, the unadmitted novelties of Vimont, the host of new organs established (to their own satisfaction) by the mesmero-phrenologists, but not admitted by their more conservative leaders, or the discrepancies between the London, Edinburgh, and Parisian professors of the science, in proof of the impossibility of drawing any safe or valid deductions from the method of observation which they advocate. And concerning the second objection we have to remark that, with regard to the broad distinctions between reflex (or excito-motor), instinctive (or sensori-motor), and intelligent (or volitional) actions—which distinctions, as we shall presently show, are of fundamental importance in the inquiry,—we think that ample materials exist for determining them, if they are rightly employed. In fact, in the prosecution of both these inquiries the great evil seems to us to have been, that we have taken man as the type, and have endeavoured to refer every thing to our own standard; whereas, if we had investigated the structure of the nervous system in the different classes of animals, and studied their actions, *from below upwards*, we are confident that much greater progress would now have been made.

In no department of physiology, as it seems to us, is the necessity of a continual appeal to comparative anatomy more obvious, than in the inquiry into the functions of the nervous centres. There is none in which we can more clearly trace the withdrawal of one organ after another as we descend the scale, leaving us a remainder whose operations we can study without the disturbing influences of vivisection. There is none in which we can more readily trace (if we only know how to look for it) the distinction between what is superadded and what is essential—what is apparent and what is real. There is none, then, in which we can so completely make use of the lower animals, in Cuvier's happy expression, as so many experiments ready prepared for us by nature. And there is none in which the neglect of this guide is more likely to lead us astray. We say this with no hostility towards the phrenological school; for, as will hereafter appear, we think ourselves able to reconcile the leading dogmata of both systems, in a degree which we have not until recently thought possible; and we have already, on several occasions, expressed our leanings in its favour. But we are desirous that all inductions respecting the physiology of the Encephalon should have the soundest possible basis; and we are convinced that the best method of ascertaining what are the functions of its several parts, is to study those conditions in which their development presents the most striking contrasts, before proceeding to the comparison of them in different individuals of the human race, amongst whom there is not by any means a like diversity. Thus we are surely better able to estimate the functions of the cerebrum as a whole, and thus to lay the foundation for the inquiry into the distinct operations of its several lobes and minor subdivisions, if we first determine what it does *not* do, by tracing it through the various degrees of degradation which it exhibits as we descend the vertebrated series, until it entirely disappears in the lowest; and by observing the actions of animals, which are destitute of the remotest analogue of it—than by any observation of even the extremest diversities, which are presented in its development in the human race. From not attaching what appears to us sufficient importance to this kind of investigation, Mr. Noble devotes his almost exclusive attention to the functions of the cerebrum and cerebellum; his allusions to the existence of any other ganglionic centres not being by any means pointed, except where he combats the opinions of Drs. Carus and Carpenter upon the functions of the tubercula quadrigemina. Consequently, though professing to be a treatise on the brain, his book is really devoted almost exclusively to an exposition of the function of the cerebrum and cerebellum alone. Nevertheless, from some hints let fall here and there, we gather that Mr. Noble is inclined to admit that the primary seat of sensation is not in the gray surface of the hemispheres, but in the masses of ganglionic matter at the base of the brain. Now, as we shall presently endeavour to show, the existence of a series of ganglia in immediate connexion with the organs of sense, is a neurological fact of far greater generality than the existence of a cerebrum, and is, therefore, of more fundamental importance; and we by no means despair, therefore, of convincing Mr. Noble and those phrenologists who like him are real truth-seekers, of the justice of our views regarding the primary functional value to be attached to these bodies.

Our present object is, rather, to indicate the mode in which the physiology of the encephalon ought, in our opinion, to be studied, than to enter upon a detailed criticism of Mr. Noble's very clever treatise; since there are many circumstances which lead us to refrain from the latter course, whilst we think we can do some service to science by adopting the former. But in justice to Mr. Noble we feel it right to state, that we know of no treatise which is calculated to convey to an intelligent and discriminative reader a better idea of phrenology; as he is alike free from the shallow dogmatisms of some authors, and the bold and ignorant quackeries which are so repulsive in others; and, if the correctness of his fundamental principles be conceded, makes a legitimate and philosophical use of observation for the establishment of the truths of which he is in search. Such might be expected from the author of "True and False Phrenology," and of other valued contributions to medical science.

There are some other points, however, which we must discuss with him, before we can altogether dismiss him from our critical bar; deeming him in special need of wholesome correction therefrom. Such, for example, we find in the mode in which he has handled the next object of inquiry,—the use of vivisections, in determining the functions of the nervous system; in commenting upon which Mr. Noble has, we think, greatly underrated the amount of information to be derived from experiments. We cannot call to mind a single point of fundamental importance, that has been established in this department of physiology without such aid. Where would have been our knowledge of the general seats of sensation and of the motor power, if the severance of the nerve-trunks had not revealed to us, that connexion through their means with the nervous centres is essential to the sensibility of every part of the body, and to the subjection of its contractility to the will? Where would have been our knowledge of the relative functions of the fibrous and vesicular (or white and gray) portions of the nervous substance, if experimental research had not informed us, that the former possesses no power save that of conduction, and that wherever the latter presents itself there is an independent power of origination? Where would have been our acquaintance with the relative functions of the anterior and posterior roots of the spinal nerves, had it not been for the vivisections of Sir C. Bell, Magendie, Müller, and others? Mr. Noble thinks that experiments can only be valuable, where there has been some antecedent knowledge gained by analytical comparison of function and structure; but all that anatomical inquiry impressed upon the mind of Sir C. Bell, before commencing his experiments, was that these roots must have different functions; and it is notorious that he had formed an erroneous idea of that difference, and that it rested with experiment to set him right. Where, again, would have been our knowledge of the different functions of the cranial nerves—of the purely motor character of some, of the mixed endowments of others—of the absence of common sensibility in the nerves of special sensation, and of many other similar particulars,—but for experiment? True it is, that minute anatomical inquiry might have rendered many of these inferences probable; but it needed the confirmation of experiment to establish them; and we cannot call to mind a single instance in which there is a discrepancy between the results of these two modes of inquiry. We might advert to the purely motor character of the facial nerve, the purely sensory endowments of the first and second branches of the fifth pair, the mixed endowments of its third division, the almost purely afferent character of the glosso-pharyngeal, the relative afferent and motor powers of the superior and recurrent laryngeal nerves, the specially motor endowments of the pharyngeal branches of the par vagum, and many other such determinations, as resting with equal security upon both these foundations. Where, again, would have been our whole knowledge of the independent endowments of the spinal cord, and of its functional analogy with the ganglionic chain of the articulata, but for vivisections? And yet, Mr. Noble would have his readers to believe that little reliance is to be placed upon them; and why? Because the inferences which have been drawn from them do not always accord; the true interpretation of the results not having always been given at first. We wonder that his penetrating mind should have overlooked the fact, that where the first inferences have been demonstrated to be fallacious, that demonstration has been afforded by *additional experiments.*

Mr. Noble quotes, among other illustrations of the uncertainty of such results, the well-known error into which Sir C. Bell was at first led, in regard to the supposed motor functions of the infra-orbital nerve. The ass, in which this nerve had been divided on both sides, stooped down to the oats that were placed before him on the ground; and though the oats touched his lip, he made no effort to pick them up: whence Sir C. Bell inferred that the power of moving the lip had been destroyed by the operation. Now we could not have a better illustration of the necessity of cautiously separating the phenomena, from the inference founded upon them by the experimenter. Every subsequent vivisector has confirmed this fact; the result of the experiment is invariable. But the inference was wrong, and is now universally admitted to have been so; for it is not the motor power which has been destroyed, but only the sensory; and the animal, not being able to feel the oats, makes no effort for their prehension. And how was this correction supplied? By the additional experiments of Magendie and Mayo; who applied other tests, and proved the persistence of the motor power whilst the sensory was abolished. What ground, then, does this, or any similar fact in the history of neurology, afford for cavilling at the results of experimental research, as uncertain and fallacious? Does Mr. Noble feel less confidence in the now universal accordance of physiologists in Sir C. Bell's determination of the relative functions of the anterior and posterior roots of the spinal nerves, because his inferences were at first incorrect, or because, even when better established, they were for a long time disputed among physiologists; or because whilst *these* are generally received, others of his doctrines (such as his idea of the relative functions of the anterior and posterior columns, or the special respiratory system of the nerves) have not gained equal currency, or are altogether rejected? In no science do we find that the whole truth is disclosed and established at once, even when the most sagacious investigators are in pursuit of it; and, considering the peculiar difficulties which attend all physiological researches, and especially those in which the nervous system is involved, we should rather feel surprised that so much has been done in the short time that has elapsed since the path of inquiry was first opened up, than sceptical as to the power of experiment and observation, judiciously conducted, to establish much of what we seek to know.

We would not be thought, however, to be over sanguine as to the results we expect from vivisections practised upon the encephalon. We have never been disciples of the school of Magendie; and we should shrink from being supposed to sanction the reckless barbarities of those, who have adopted him as their master and guide. We do not disguise from ourselves that any disturbance of the proper balance of pressure in the contents of the cranium is likely to affect the condition of every part; and that it is difficult, if not impossible, to effect the entire severance of any part from its connexions with the rest, without thereby disturbing the normal functions of those which remain. We cannot, therefore, feel the same confidence in the results of vivisections practised upon the encephalon, taken alone, as we should upon those of the division or irritation of a nerve. But if the results of experiments tally sufficiently well with the influences which seem to flow legitimately from comparative anatomy,—if we find an animal, for instance, from which a certain organ has been removed, to be reduced to the same level with one which is naturally destitute of it, allowance being made for the disturbance produced by the immediate shock of the operation,—or if we find that the removal of another organ uniformly destroys a power, which seems to be possessed by different animals in a degree that accords with the development of that organ,—we unhesitatingly say that it must be very cogent and well-founded evidence, which should lead us to put aside such a determination. The value which we attach to vivisections upon the *brain*, may not, after all, be very different from that set upon them by Mr. Noble; since we have confidence in their results, only when they have been made in harmony with the structural divisions pointed out by nature, and under the guidance of a reasonable idea of the functions of the parts experimented on. But *we* should found these preliminary ideas, both as to structure and function, upon the wide and (in our opinion) secure basis of compative anatomy; which Mr. Noble would attempt to exclude, as affording no sufficiently positive data. Whether it does so, or not, time and good sense must decide.

Another point on which Mr. Noble has fallen into mistake, through the want of a sufficiently extensive acquaintance with comparative anatomy, and on which we believe that he participates in a common error (which therefore calls the more loudly for correction), is the following. In replying to objections raised against the phrenological system, on the ground that it is unsustained by any direct anatomical proof, he says, (p. 237), "Objections such as these can only arise from singular thoughtlessness, or from utter ignorance of nature's plan in the development of the nervous system, which, *as a rule, exhibits no mechanical divisions coincidentally with distinctness in functions.*" [The italics are Mr. Noble's.] We do not overlook the connexion in which this sentence occurs; from which it may be inferred that Mr. Noble had the cerebrum of vertebrated animals alone in his mind, and merely intended to account for its not being mapped out like a phrenological bust. But the prominence given to the latter part of it is certainly calculated to mislead; for the applicability of the statement is limited to vertebrated animals, the *general* fact being very different. For no one can take even a cursory view of the development of the nervous system throughout the entire animal series, of which the invertebrated classes form *by far the largest part*, without perceiving the almost invariable rule in those classes to be, that mechanical division of the nervous centres *do* coincide with distinctness of function. We have ganglia for respiration, ganglia for mastication and deglutition, ganglia for locomotion, ganglia for special sensation, and ganglia for various actions peculiar to the species (as those connected with the suckers of the cuttle-fish), all of them distinct from each other, and easily recognized by their connexions with the organs specially destined to these functions respectively. In the higher forms of some of the most elevated classes, both of the articulated and molluscous series, we do trace that tendency to fusion and concentration, which is characteristic of the vertebrated sub-kingdom; but even in them the original mechanical distinctness of the ganglia that are functionally different, may be traced by sufficient attention to the history of their development. Thus the large ganglionic masses which occupy the thorax in many insects, and which are almost the only nervous centres in their trunks, are clearly formed by the fusion of some of the ganglia that were distinct in the larva, and by the increased development of the compound centres thus formed, to the exclusion of others which diminish and even disappear altogether. Now in the vertebrated series, which, in any general survey of this kind, must be considered as only *one* out of *four* distinct types, we find a still greater tendency to fusion or union of different ganglionic centres; but the purposes of this are evident,—to place them all under the protection of the cranio-spinal envelope, and to bring them into closer relation, in order to secure greater consentaneousness of action. But does not every physiologist know that in spite of external indications, real mechanical divisions still exist? Are not the segments of the spinal cord as independent of each other, in regard to their power of ministering to the reflex actions of the nerves proceeding from them, as are the distinct ganglia of the ventral cord of the articulata? Is the tract of vesicular matter, which forms the centre of the respiratory nerves, less a respiratory ganglion, because it is imbedded in the medulla oblongata? Is the gray nucleus, into which the root of the auditory nerve may be traced, less a mechanically distinct auditory ganglion because it happens to be imbedded amongst strands of white fibres connecting the organs above and below it?

But even were we not allowed to push our anatomical inquiries so far, as to determine by their means that what bears the external aspect of a single ganglionic mass is in reality formed by the fusion of several, the argument is all the stronger for the functional distinctness of those parts, which *do* remain mechanically distinct, even in the vertebrated series; and we cannot but think that we shall be better guided in our inquiries, by following out the clue afforded us by the divisions which nature has established, and of which she still leaves us the external indications, than by setting out with the establishment of new and arbitrary divisions of our own. Surely the condition of the sympathetic system could never have occurred to Mr. Noble's mind; presenting to us, in the highest vertebrated animals, precisely what we have represented as the *most general* condition of the nervous centres, viz. functional independence, combined with mechanical distinctness. It is not strange that, in the infancy of comparative neurology, the sympathetic system of vertebrata should have been compared with the entire nervous system of invertebrated animals, consisting (as both do) of ganglia scattered as it were through the entire body. Nor is it difficult, on the other hand, to see why the sympathetic system should retain that condition, even in the highest vertebrata, since its offices are not of such predominant importance as to require the protection of the cranio-vertebral column; nor is there such need of consentaneousness in the actions of its different parts, as to require that close connexion between them, which we see so remarkably effected by the union of the centres altogether making up the cerebro-spinal apparatus.

We have made these remarks at this stage of our inquiry because, as we shall next endeavour to show, there is a much greater amount of mechanical distinctness (we use Mr. Noble's phrase in default of a better) in the encephalic centres of man and the higher vertebrata, than is generally supposed. We cannot have a better opportunity of demonstrating the importance of a due acquaintance with comparative anatomy to him who attempts to specialize the functions of the different parts of the human encephalon, than by contrasting the ideas we are led to form of the fundamental divisions of this complex organ, by the study of its anatomy in man alone, with those which we gain from a more extended survey of its development, in the series of animals constructed upon the same general model. The attention of every mere human anatomist (and we are not aware of any phrenologist who forms an exception to this statement) has been fixed almost exclusively upon the two organs, distinguished as the *cerebrum* and the *cerebellum*, as forming, with the medulla oblongata, the entire encephalon. It is not surprising that it should be so, when the preponderating bulk of these organs is considered, and when it is further borne in mind, that these are the only masses which *appear* to have any distinctness—the only ones, indeed, which can be proved to possess any speciality, except by the very closest scrutiny, guided by the light thrown on the search by comparative anatomy. But, go to the opposite extremity of the vertebrated series, and examine the encephalon of a fish: what is there to be found? A series of at least *four* distinct ganglionic masses, arranged in a line continuous with the spinal cord, three of them in pairs, the fourth and hindmost single. The analogues of these in the human encephalon cannot be determined without the most careful scrutiny, both into the relative connexion of the parts, and into the relative degrees of development, and the changes of form and position, which they present in the several intermediate groups, which connect the lowest with the highest of the vertebrated series. It cannot be said that this study, the importance of which has only been recognized of late years, has yet been prosecuted in such a manner as to place every analogy in its clearest and most definite form; but the following points may, we think, be regarded as established by the general consent of those most competent to form a judgment on the subject.

1. The most anterior of the ganglionic masses in the encephalon of the fish, on either side of the median line, is the *olfactory* ganglion, being the centre in which the olfactory nerve terminates. Its connexions are with the most anterior extremity of the medulla oblongata, no evident communication of a direct character being traceable between the olfactive ganglion and the masses which lie immediately behind it. Not unfrequently we find that the olfactive ganglia are separated by a considerable interval from the remainder of the encephalon, being carried forwards in order to be in more immediate apposition with the organ of smell; and being connected with the medulla oblongata by slender peduncles. Hence they have been frequently overlooked, and the peduncles have been mistaken for nerve-trunks, proceeding from the second pair of ganglionic masses. This mistake has been common in human anatomy also; the real ganglionic character of the bulbous expansions, which lie on the cribriform plate of the ethmoid bone, having been overlooked, and the peduncles or commissures, which connect them with the other parts of the encephalon, having been consequently ranked as the first pair of nerves. There can now, however, be no doubt or mistake about the matter, since the presence of vesicular matter in these bulbous expansions, even in man, clearly demonstrates their ganglionic character; and in many of the lower mammalia, in which they are relatively of much greater size, they include a distinct ventricle.

2. The second pair of ganglionic masses in the brain of the fish is now regarded, we believe, by all the most eminent anatomists (whatever may have been their former differences of opinion), as the sole representative of the *cerebral hemispheres* of the higher vertebrata. But even this it is not *in toto*, for the fact appears to be, that only the external lamina of each ganglion, which in most fishes is very thin, can be at all compared with the cerebral hemisphere, the interior mass being rather to be considered, from its aspect and connexions, in the light of a corpus striatum. And we shall presently find reason to believe, that this rudiment of a cerebrum does not represent the entire organ, any more than the early embryo can be regarded as a complete *homunculus*, but that it must be considered as the first indication of the presence of the *anterior lobe* alone.

3. The third pair of encephalic masses has received the name of the *optic lobes*, being evidently the ganglionic centres of the optic nerves. On closely scrutinizing their structure and connexions, it appears that they contain the analogues of the corpora quadrigemina and thalami optici of higher animals, the roots of the optic nerves being especially connected with the former, which must be regarded as the representatives of the optic ganglia of the invertebrata.*

---

* The first attempt at a philosophical comparison between the encephalon of vertebrated and invertebrated animals, we believe to have been made by Mr. Owen in regard to the brain of the cephalopoda; in the illustrated catalogue of the Museum of the College of Surgeons, vol. i, 1835; and in his article "Cephalopoda," in the Cyclopædia of Anatomy and Physiology, vol. i, 1836. In 1838, Mr. Newport, in his prize essay upon the Athalia centifoliæ, stated it as his opinion, that the cephalic ganglia of insects are analogous to the tubercula quadrigemina of the brain of vertebrata; a statement fundamentally true, but requiring (as we shall presently see) some qualification, since the cephalic ganglia are made up of at least four coalesced pairs, of which the optic ganglia form only one.

4. Lastly, we find a ganglionic mass placed on the median line, over that space left by the divergence of the fibrous strands of the medulla oblongata, which constitutes the fourth ventricle. This mass is evidently the analogue of the *cerebellum.* It sometimes presents the rudiments of lateral appendages; and, as we shall hereafter see, it varies more in its relative dimensions in different species of fish, than does any other of the encephalic ganglia.

Now, that the same distinction of parts exists in the human encephalon is clearly proved by the fact, that all these—with the exception of the olfactive ganglion, which always exists in an almost rudimentary state—are to be most clearly made out in the early embryo. The whole condition of the encephalon (making allowance for the difference between an embryonic and an adult structure) is most remarkably accordant with that of the encephalon of the fish, for it is composed of a series of rounded masses, placed in a curved line, which is continuous with that of the spinal cord, and none of them has any evident predominance over the rest. Of the most anterior of these, the superficial portion is the rudiment of the cerebral hemispheres, whilst the deeper-seated part of the mass is evidently the representative of the corpora striata. In place of the single pair of ganglia behind these, which usually combines in fishes the analogues of the corpora quadrigemina and thalami optici, we have first a vesicle within which the thalami optici and the third ventricle are formed, and then another, which gives origin to the corpora quadrigemina. The mechanical division is here more complete, therefore, than that which prevails in most fully developed fishes, though there are some, as the lamprey, which retain more of the embryonic type, and present, even in their adult condition, two distinct lobes for the thalami optici and corpora quadrigemina. Behind the vesicle from which the latter are formed, in the human embryo, is a large vesicle, within which the cerebellum is developed.

We have, then, in fishes, and in the early human embryo, this remarkable condition of the encephalic centre,—that its great mass is evidently composed of a series of distinct ganglionic centres, of which the portion representing the cerebral hemispheres is usually the smallest. We have—1st, the olfactory ganglia; 2d, the corpora striata, covered by a thin lamina, which is the rudiment of the cerebral hemispheres; 3d, the thalami optici, inclosing the third ventricle; 4th, the corpora quadrigemina; and 5th, the cerebellum. *There is no more general fact in the whole range of comparative anatomy*, than that the encephalon of vertebrata is composed of these elements at the commencement of its development; and however much their relative size and importance may be altered in the progress of their subsequent evolution, their original distinctness remains unaltered. Even in the adult human brain this distinctness may be recognized by careful dissection; for, if we take the medulla oblongata as our guide, and trace its connexions with the several ganglionic masses at its summit, we find it sending distinct fasciculi of fibres into the cerebellum, the corpora quadrigemina, the thalami optici, the corpora striata, and the peduncles of the olfactory ganglia. The cerebellum is not more "mechanically" distinct from the cerebrum than are the corpora quadrigemina, the thalami optici, and the corpora striata from the true cerebral hemispheres. Before attempting, then, to determine the functions of the compound mass, which, taken as a whole, we are accustomed to designate as the encephalon, it appears to us essential that we should distinguish the relative offices of the true cerebral hemispheres, and of the chain of ganglia which they inclose and overtop. We shall otherwise be in continual danger of attributing to one what are really the attributes of the other; and of reasoning upon that as a whole, which is really made up of a series of parts, alike mechanically and functionally distinct. Common sense, we apprehend, would dictate to us, that in the study of a complex piece of machinery, we should first make ourselves acquainted with the actions of the parts, which are sufficiently isolated to enable us to observe their operations independently of those of the remainder, and to put aside from the sum total all that can be fairly attributed to these, before endeavouring to analyse the operations of the portion that most defies our scrutiny. It is absurd to suppose that there is any such complete alteration in man of the relative functions of the several parts, as shall do away with the necessity for regarding them as distinct; when the whole history of the development of the human encephalon indicates its precise accordance with the general type of structure which prevails in the nervous centres of vertebrata, and when nothing is needed but careful scrutiny, to discover the same distinctness in these parts in the *complete* brain, as in the *embryonic*.

We must not, however, quit our account of the fish's encephalon without remarking that, although the ganglionic masses we have enumerated are those most constantly distinguishable, there are others which are probably always present, though usually *fused* into the medulla oblongata,—such as the ganglia of the auditory nerve and of the par vagum, which are "mechanically" distinct in some species, especially those of the carp kind,—whilst there are others, again, which are evidently called into existence to supply some special requirement. Under this last category we may rank the electrical lobe, an additional ganglionic mass peculiar to the gymnotus, from which proceed the greater part of the nerves that supply the electrical organs; a median ganglion, occupying nearly the same situation, but having a function (as known by its connexions) entirely different, in the remora, or sucking-fish, the remarkable suctorial disk on the head of which is supplied from it; and a series of ganglionic enlargements in the medulla oblongata of the trigla, or gurnard, which supplies nerves to the peculiar digitiform prolongations of its pectoral fins. We have noticed these facts to show that the connexion between "mechanical distinctness" and diversity of function is still a very obvious one, notwithstanding the tendency to the fusion of different parts of the nervous centres, which is generally so remarkable in vertebrata.

If, then, we consider the encephalon of vertebrata under its *most general* aspect—that, namely, which all vertebrated animals present at an early period of their development—we find it to consist essentially of the following parts. In the first place, of the medulla oblongata, or cranial prolongation of the spinal cord, which usually includes the respiratory, auditory, and gustatory ganglia, and of which the several strands have distinct connexions with the other ganglionic masses in the encephalon. Secondly, of olfactive and optic ganglia, whose functions are known by their immediate connexion with the nerves of those senses respectively. Thirdly, of masses representing the thalami optici and corpora striata of the human brain, which are here seen to have an existence quite independent of that of the cerebrum, instead of being mere appendages of it. Fourthly, of the cerebellum. And fifthly, of a rudiment of the cerebral hemispheres. When it is borne in mind, that of the second pair of ganglionic masses in the fish's encephalon (which is very frequently smaller than the optic lobes, and seldom much larger) only the external layer or crust is the representative of the real cerebrum, we at once see what a very subordinate part it must perform in the sum-total of the operations of the nervous centres of these animals. The main instruments of these operations must evidently be the *chain of ganglia connected with the sensory organs*, in which, for the reasons we shall presently mention, we include the thalami optici and the corpora striata; and the encephalon is found to be more and more exclusively made up of them, the lower we descend in the scale, until the rudiments of the cerebral hemispheres disappear altogether.

We shall next briefly inquire into the analogues of these organs in the invertebrata, with the view of showing how uniform is the plan on which the encephalon is constructed in the whole animal kingdom. Wherever we have a distinct *head*—that is, wherever we find the mouth placed upon a prominent part of the body, which is furnished with organs of sense adapted to the selection of the food and the direction of the movements of the trunk—there do we find a pair of cephalic ganglia, which evidently have somewhat of the same dominating influence over the whole remainder of the nervous centres, as that which is possessed by the encephalon of vertebrata. That influence, however, seems to be often much less powerfully exerted; most of the movements of the body being due to the simply reflex action of the ganglia in immediate connexion with the several organs, any one pair of which frequently surpasses in size the whole of the cephalic mass. But in proportion to the development of the organs of sense in the head, do we find the size of the cephalic ganglia increasing; and, when perfect and complex organs of vision are present, as in the higher articulata and mollusca, we find the optic nerves attaining a very large size, whilst the cephalic masses are evidently in great part to be regarded as the ganglionic centres of those nerves. This becomes particularly obvious in watching the metamorphoses of insects, as Mr. Newport long since pointed out. For there are many larvæ which have very imperfect eyes, whilst the perfect insect possesses fully developed organs of vision; and the enormous increase in the size of the cephalic ganglia, which takes place during the metamorphosis, obviously corresponds with that development. But we must not regard the cephalic ganglia of insects, &c., as entirely analogous to the optic ganglia of fishes, since it is obvious from their connexions, that they are also the ganglionic centres of other organs of sense. And as, in the higher invertebrata, the enormous development of the optic ganglia causes all the rest to be swallowed up into them (so to speak)—just as the great development of the cerebral hemispheres in man obscures many other parts of his encephalon—it is needful to go to the simpler, and, above all, to the embryonic conditions of the encephalon, to discover their real signification. Now, when the head of a myriapod is examined in an early stage of its development, as Mr. Newport has so admirably shown,* we find its encephalon in a condition so precisely analogous to that of the lower fishes, that it is impossible not to be struck with the correspondence. The head consists of two principal divisions—the *cephalic* proper, and the *basilar*,—each of them composed of four segments. Each of these segments has its pair of appendages, and each possesses its own pair of ganglionic centres. The four ganglia of the cephalic portion subsequently coalesce to form the cephalic ganglia of the perfect animal, whilst the four of the basilar portion coalesce to form the first subœsophageal ganglia. Now, the four pairs of ganglia, included in the cephalic, lie one behind the other in regular succession, like the ganglia of the encephalon of the fish, and are just as functionally distinct; for the first pair is that which furnishes the *antennal* nerves, and the second the *optic;* the third pair supplies the internal maxillæ, and the fourth pair the maxillary palpi. Thus, the cephalic ganglia of the adult animal combine the functions of three pairs of ganglia, which are connected with distinct sensory organs; and is thus obviously analogous to the chain of sensory ganglia, which makes up by far the largest part of the encephalon of the fish. There is here, as might be anticipated from the gradual disappearance of those organs in the lowest fishes, no rudiment of the cerebral hemispheres, which are at once distinguished from other ganglionic masses by their *superadded* character, having no direct connexion with any of the nerves, but being implanted, as it were, upon the summit of the strands which pass upwards from the nervous centres of the trunk. Such a rudiment seems to present itself, however, in the encephalon of the highest mollusca, the cuttle-fish tribe, which present so near an approach to fishes in the development of individual organs, although the plan of their arrangement is different; for, in addition to the highly developed optic ganglia, and the ganglia from which the arms, masticating apparatus, and other parts are supplied, there is a single cordiform mass, placed on the median line, which, from its position and connexions, may be looked upon as the nearest analogue of the cerebrum that is presented among the invertebrata. It is worthy of remark, in reference to the position in which we nearly always find the auditory ganglion of vertebrated animals (which is imbedded, as it were, in the medulla oblongata, on either side of the fourth ventricle), that the first rudiments of the *organ of hearing*, which present themselves in the gasteropod mollusca, themselves occupy this very situation; being a pair of minute sacs, containing numerous minute *otolithes* floating in fluid, which are imbedded in the posterior lobes of their cephalic ganglia, which answer to the medulla oblongata of vertebrated animals.

Quitting, now, these forms of the nervous system, which represent the encephalon of the vertebrated animal deprived of its cerebrum, let us briefly examine into the nature of those more complex forms, which are characteristic of the higher vertebrata. It is not enough to pass at once from the encephalon of the fish to that of man. The true character of *his* brain cannot be known without a patient and gradual survey of the whole ascending series, nor without a careful comparison of the gradations thus encountered, with those which are presented to us in the progress of embryonic development in the most perfect and complex form. A very cursory examination yields to us this important deduction,—that the increased complexity of the encephalic structure, in the higher vertebrata, is almost entirely due to the extraordinary development of the cerebral hemispheres, and of their commissural connexions with the other ganglionic masses; the development of the sensory ganglia not making by any means the same proportional advance, or rather, advancing rapidly up to a certain point, and then ceasing. A new and special series of observations are much required, for the sake of determining the relative sizes of the several organs we have indicated in different animals, both with respect to each other and to the bulk of the body. Thus, it is well known that the general statement, that the brain of man is larger in proportion to the entire weight of his body, has been disproved by the fact that a higher proportion is presented by several small birds and rodens. But if the statement had been made, that the *cerebrum* of man is heavier in proportion to the weight of his body than that of any other animal, we apprehend that it would have been strictly correct. For the encephalon of birds is still made up chiefly of the chain of ganglia we have described, which attains, we believe, its highest development, relatively to the weight of the body, in that class. On turning aside the cerebral hemispheres (which do not yet altogether cover in the optic lobes), we find that they really form but a thin lamina, beneath which lies a series of large protuberances, which the study of their connexions at once discovers to consist of the corpora striata, thalami optici, and optic ganglia. If these were weighed, exclusively of the cerebrum proper and cerebellum, we are assured that they would bear a larger proportion to the whole body of the smaller birds, then they would in any other class.

---

* Linnæan Transactions, vol. xix; see also our Twentieth Volume, p. 493 et seq.

Before passing from this subject, we must notice an extraordinary error, into which Mr. Noble has been led whilst criticising certain opinions of Dr. Carpenter's, in reference to the functions of the corpora quadrigemina of man. "In birds," he remarks, "the optic tubercles (the *nates*) alone are found; the certain analogues of the *testes* do not exist;" whence he infers that Dr. Carpenter must be wrong in attributing to these organs any share in the instincts and emotions, having demonstrated to his own satisfaction that such a class of functions must be limited to the *testes*, if appertaining to the corpora quadrigemina at all. And, as birds have instincts and emotions, but no *testes*, Dr. Carpenter must be wrong in his allocation. Now we shall hereafter discuss the general question of the connexion of the instincts and emotions with the sensory ganglia, and shall only stop to point out the fallacy of Mr. Noble's argument. The *corpora bigemina* of birds are actually *far larger* in proportion than the *corpora quadrigemina* of mammals; and although the duplex enlargements of the latter might lead to the idea that these organs receive an increased development in that class, yet there is no proof whatever that the peculiarity is anything but superficial, the connexions of the optic ganglia in mammals and birds being exactly alike. We shall presently find that the *very large* proportional size of the optic ganglia in birds is an important argument in favour of the very doctrines of which Mr. Noble, (doubtless owing to having derived an erroneous notion of them, from the imperfect manner in which they were first expressed by Dr. Carpenter) has given, in sundry parts of his work, a very strange misrepresentation.

The development of the cerebral hemispheres in the ascending vertebrated series, and in the human embryo, has lately been elaborately investigated by Professor Retzius of Stockholm; and he has given the most complete confirmation of the statements of former comparative anatomists in regard to the order in which the different lobes of the cerebrum are evolved, whilst he has shown that the same order prevails in the embryological development of the human encephalon. Notwithstanding the contradiction of Mr. Noble and the phrenological luminaries whom he quotes, we rely upon the testimony of those who *have* studied Nature and put her to the question in this matter, for the universal truth of that which a very limited amount of observation serves to indicate; namely, that the part of the cerebrum which is *most* developed in man, in comparison with other animals, is not the anterior but the *posterior.* This was long ago asserted by the opponents of Gall, upon the ground that the cerebrum of none of the lower mammalia extends so far backwards as to cover the cerebellum; and the objection was met by the phrenologists with the assertion that there is no anatomical division between the middle and posterior lobes, and that it is quite conformable to our general knowledge of the disposition of the several parts of the encephalon to suppose, that the parts corresponding to the posterior lobes of man do exist in the cerebrum of the lower mammalia, but that they are not extended so far backwards, because the whole mass of the cerebrum in man is pushed backwards by the increased development of the anterior and middle lobes. Now although there is no external line of separation between the middle and posterior lobes, such as exists between the anterior and middle, there are two points of internal structure which afford as definite an indication as can be required; these are the development of the posterior cornua of the lateral ventricles, and the situation of the hippocampus major. By these means it may be unequivocally determined, that the development of the *posterior* lobe of the human cerebrum is far greater in proportion to the whole mass, than it is in the highest of the other mammalia; and that in the lower mammalia, as in all inferior vertebrata, *the posterior lobe is totally wanting.* Further, the philosophical anatomist well knows that the rudiment of a cerebrum which exists in fishes, represents the *anterior lobe only;* that this enlarges as we ascend through the classes of reptiles and birds, but does not change its character; that the middle lobe is only developed as we enter the mammalian class, presenting itself at first in a very rudimentary form, and attaining increased development as we ascend; and that the posterior lobe is developed from the back of the middle lobe, making its first appearance in the carnivorous group.

This history *finds an exact parallel in that of the embryonic development of the human cerebrum;* the rudiment which presents itself at a period when the chain of sensory ganglia has attained an advanced development, having been shown by Professor Retzius to be the representative of the anterior lobe only, the development of this making considerable progress before the middle lobe begins to be evolved, and the posterior lobe being the latest in order of evolution. Strange as this assertion may be to phrenologists, we have, nevertheless, an implicit confidence in its truth, arising from the knowledge which personal acquaintance has lately enabled us to form, of the remarkable perseverance and sagacity with which the eminent professor of Stockholm pursues his investigations, and of the habits of philosophical deduction which he exercises in drawing conclusions from his observations. The following summary of these we extract from the Monthly Report of the Royal Academy of Sciences at Stockholm, to which they have been recently communicated. We may mention that they fully coincide with the observations made by Tiedemann thirty years since; which were, however, neither so precise nor so complete.

> "In the first period, which corresponds with the second and third months, only, the anterior lobes form; in the second period, which is comprised in the end of the third month, in the fourth, and in a small portion of the fifth, the two middle lobes appear; and after this time the posterior lobes. During the first period the descending horns of the lateral ventricles and the pedes hippocampi are wanting; these are added in the second period. During a great portion of the first period the hemispheres do not cover the thalami nervorum opticorum; in the second period they completely overlap these parts, approach the large corpora quadrigemina, cover their anterior part, and then descend by the side of the cerebral nucleus (cone or stem), and, as it were, fold round it. If we examine a brain at this period of development we might, from its external appearance, imagine that the posterior margin of the hemispheres corresponds to their persistent posterior ends and margins, i. e. to those which are their posterior margins in their perfectly developed state. But it is not so. If we open the brain we come at once to the descending horns of the lateral ventricles, in which are the rudiments of the great pedes hippocampi. At a later period, in the fourth month, a small superficial notch is formed at the posterior margins of the hemispheres; and that part of this margin which is above the notch is the first rudiment of the posterior lobes of the hemispheres. These, which are thus for a long time only rudimental, begin above the middle lobes, gradually take in their posterior margin, follow it down, as development advances, by the sides of the cerebral nucleus, and terminate at that part of the middle lobes which meets the pedes hippocampi. Even in the brain of the mature fœtus, as well as in the fully developed brains of older persons, the posterior lobes are very clearly separated from the middle lobes by a branching furrow, which is especially distinct, on the vertical side of the hemisphere which lies next to the falx."

It is interesting to observe that these researches find a parallelism in another sub-kingdom; for they coincide as to the direction of the development with the account which has been already given by Mr. Newport, in the Linnæan Transactions, of the manner in which the *cephalic* portion of the head is developed in the myriapod and other articulata, by an extension of the coalesced segments of which it is formed, from before backwards, so as to overlap the basilar portion, in exactly the same way as the cerebrum is now shown by Professor Retzius to overlap the corpora quadrigemina and cerebellum.

Now we affirm that no phrenologist is entitled to dispute such a position, without having followed up a similar investigation with similar perseverance and with equal capacity. And at present the explanation to which we just now alluded, as having been offered by the followers of Gall, is capable of being turned pointedly against themselves; for if, as they maintain, the increased development of one lobe of the brain may cause the displacement of another, the fullness of the *forehead* in man may be attributed to the development, not of the *anterior* lobes of his cerebrum, but of the *posterior*, these being the last to be evolved, and the parts whose superior development is most marked in him.

Following out our principle of endeavouring to determine, in the first instance, the functions peculiar to divisions of the nervous centres, whose "mechanical distinctness" is obvious, we shall proceed to inquire into the relative endowments of the cerebral hemispheres, and of the ganglia of sensation: and shall investigate the evidence afforded on this subject; first, by comparative anatomy; and, secondly, by experimental inquiry. Now the first remark that is forced upon us, in the most cursory examination of the structure of the cephalic ganglia of different animals, is this; that the *essential* part of the encephalon is formed by the collection of sensory ganglia, and that the cerebrum is an organ *superadded* for some purpose, which is only fully attained in the higher vertebrata. We see the cerebrum dwindling away as we descend the scale; until, in the lowest of the class of fishes (the amphioxus, or lancelot) no rudiment is left of it. But we find no such corresponding diminution in the sensory ganglia, even in those which have been commonly most regarded in the light of appendages to the cerebrum. And on proceeding to the invertebrata, we find the absence of the cerebrum to be the almost invariable rule, even where the sensory ganglia attain their highest development. We never find a cerebrum without the sensory ganglia; but, *in by far the greatest number of animals, we find the sensory ganglia without the cerebrum;* and it is only in mammalia, that the development of the cerebrum is such, as to give to it any obvious predominance over them.

We think ourselves fully entitled, then, to infer from these facts, that *the cerebrum is not the seat of sensation*; since we must otherwise believe that those animals which are destitute of it—insects, for example—are destitute of sensibility. In like manner, we may infer that the several classes of impressions are communicated to the consciousness through the medium of the ganglia, in which the nerves that convey them terminate; the optic ganglia, for instance, being the seat of visual sensations, the auditory, of the sensations produced by sounds, and so on. For there is no single organ that seems to be sufficiently the centre of the rest, for us to regard it as the special sensorium. Guided by this view, it seems to us not difficult to assign a function to the thalami optici; for whilst we have distinct olfactive, optic, and auditory ganglia, it seems but reasonable to expect, that we should have a ganglion for the formation of the sensations communicated by the nerves of touch; and the connexion of the thalami with the spinal cord into which all these nerves pass, is precisely what we should anticipate on such a view of its offices. Moreover it is interesting to remark that the connexion between the thalami and the proper optic ganglia is everywhere very close; and this, again, harmonizes well with the fact which is familiar to every one, that the sensations of sight and touch are more closely associated, both in the origination of mental *perceptions* (or general notions of the characters of the objects of sensation) and in the direction of muscular movements, than are any other pair of sensations.

But we think it may be further asserted with confidence that the ganglia in question are the sources of all those movements, which, following directly upon sensations, may be termed *consensual.* This is a class of actions which has, we think, been too little scrutinized by physiologists; and we are much mistaken if, under the guidance of comparative anatomy and experiment, combined with pathological observation, the subject is not capable of considerable elucidation. We must content ourselves with here briefly indicating the outline of the doctrines, which have been long in progress of development in our own minds, and which have now attained a completeness and consistency, that warrants our expressing them with some degree of confidence. Putting aside the purely reflex actions, in which a movement is directly excited by an *impression*, without the intervention of a *sensation*, a little observation of our own corporeal phenomena will bring under our notice many instances, in which movements are excited with equal directness and constancy by *sensations*, without the intervention of anything that can be properly called an *act of mind*, the simple *consciousness* being alone requisite. We may take, as illustrations, the closure of the eyelids to a dazzling light, the sneezing which is excited by the same cause, the start produced by sudden loud sounds, the vomiting induced by certain nauseous tastes or odours, and the movements of the respiratory muscles (as in laughter) which are brought on by the sense of tickling. We have purposely adduced examples of the class of movements in question excited through each of the senses, in order to show how universal is the principle for which we are contending. Knowing, as we do, that the sensory ganglia not only receive the sensory nerves, but are connected, by their implantation on the fibrous tracts of the medulla oblongata, with the motor system, we can at once understand the channel through which sensations should thus produce movements, without involving any higher act of the mind, or any exertion of the will. In the case of common or tactile sensation, there seems good reason for regarding the corpora striata as the motor portion of the ganglionic mass, of which the thalami optici constitute the sensory; the relation between them being, as well pointed out by Messrs. Todd and Bowman, the same as that which subsists between the anterior and posterior peaks of vesicular matter in the spinal cord.

Now if we inquire what movements in the lower tribes of animals are analogous to these consenual actions in ourselves, we think that a little consideration will make it at once apparent, that we are to refer to this category all those movements which are purely *instinctive.* We have on a former occasion pointed out the general characters, by which these actions may be distinguished from the volitional (Vol. XI, p. 96); and we need here only refer to their constant and immediate dependence upon sensations, and the absence (so far as we can at all discern) of anything like *an idea* of the purpose, towards which their operations are directed. We can, in fact, discover no middle view that can be taken, between the supposition that the purely instinctive actions (in which category we must include nearly the whole of those performed by insects) are immediately and necessarily prompted by sensations; and the notion that the sensations excite ideas, that the ideas are made the subject of reasoning processes, and that these reasoning processes issue in an effort of the will, *designedly directed* (as in almost every action of adult man) towards the attainment of the object. Now if this be the case, it leads us to an important view respecting the mechanism, so to speak, of instinctive actions. Our usual method of reasoning upon these subjects leads us to class these actions as manifestations of different instinctive propensities; and a learned entomologist has enumerated between thirty and forty instincts which must be attributed to the bee. Upon phrenological principles all these ought to have their separate location in its minute cephalic ganglia. But we fearlessly assert that this classification of the several actions in question, as manifestations of different propensities at all corresponding with those of which we are conscious in ourselves, is entirely of our own making, and has no real existence in any brain but our own. We might as well expect to find a distinct organ in our own encephalon, for the consensual movement of the eyes, for laughter, sneezing, or any other association of muscular actions called into play by the direct agency of sensation. The sensory ganglia are, evidently, taken as a whole, the instruments by which the instinctive or consensual actions are performed. But why should we suppose different parts of the optic ganglion to be concerned, when the sight of an object of food prompts the movements requisite to obtain it, and when the sight or sound of one of the opposite sex calls into action the muscles, that bring the insect into close relation with it? What is needed is a ganglionic mass for the reception of sensations and for the origination of muscular movements; *what* movements shall follow *what* sensations, must be determined, according to the expression of Prochaska, by the "laws written upon the nervous pulp," just as in the case of reflex actions. We see no reason to doubt that insects and other animals, whose actions are chiefly instinctive, possess feelings of pain and pleasure in connexion with particular sensations, resembling those which *we* experience in connexion with the sensations that excite consensual actions in ourselves. But we can find less ground for the belief that they possess feelings of *desire* for any objects, analogous to those which form so important a part of our own mental operations. For these feelings result from a distinct *idea* or *conception* of the object; and their purpose is, evidently, to prompt to the reasoning processes which are required for its attainment. We do not *feel a desire* to close our eyes against a strong light, unless our attention be directed to the matter, and we attempt to resist the natural action which is prompted by the sensation. We do not *feel a desire* to start on a loud and sudden sound; on the contrary, the action *necessarily* and immediately results from the sensation, unless we have expected the sound, and put the will strongly in action to resist the impulse. We do not *feel a desire* to vomit when we taste or smell anything nauseous, for our desire is naturally the other way, and nothing but the strength of the sensation can overcome it.

This class of phenomena receives a remarkable illustration in certain morbid conditions of the system. Every one of our readers must have witnessed cases of unusual susceptibility to light, sound, tactile impressions, &c., evidently originating, not in disordered conditions of the organs themselves, but in affections of those parts of the sensorium, which are the centres of their respective nerves. A very instructive case of this kind, in which a variety of curious movements were directly excited by sensations, has lately been recorded by Dr. Cowan.* We trace the same influence in hydrophobia, in which the convulsive paroxysms are brought on by the *sight* or the *sound* of liquids. And we have another very instructive illustration, in the act of yawning, which, when the encephalic centres are in a state of preparation, induced by previous fatigue, is excited simply by the *sight* or the *sound* of the action in another person, without the attention being at all directed to the sensation, or any idea being attached to it. Upon phrenological principles, this action must be referred to a propensity to imitation, which has its local habitation in the cerebrum; but we cannot ourselves perceive any reason whatever for regarding it in any other light than as a consensual action, in which the optic or auditory ganglion (as the case may be) is alone concerned.

Our location of the centre of the actions of this class, in the cephalic ganglia, to the exclusion of the cerebrum, has been made upon grounds derived from comparative anatomy alone; but we shall find it completely borne out by the results of experimental inquiry upon the higher vertebrata. In addition to the general observations which we have already made upon this subject, we think it well to add one or two remarks upon the special applicability of experiment, to the determination of the functions of the encephalic centres. In the first place, we freely admit that no good purpose whatever can be derived from any hap-hazard incisions or slicings, or from the partial removal of any single organ. But we do affirm that, if we take the guide which Nature affords us, and remove *entire* organs, whose distinctness she herself indicates to us in language not to be mistaken, we may obtain important information, confirmatory of that which we derive from other sources. But it is to be steadily kept in view, that here, as in other instances, when we bring together and compare the results obtained by different inquiries, we must be careful to separate the *phenomena* which actually presented themselves, from the inferences drawn from them by those who have recorded them. By so doing, we shall be able to reconcile many apparent contradictions; and shall find a much greater amount of accordance, than was to be anticipated from a cursory survey of the apparently discrepant accounts. Of this we shall presently have a striking instance, in reviewing the results of experiments upon the cerebellum. Moreover, we must draw a marked line of distinction between the *positive* and *negative* value of the evidence we accept. If we find that, after the removal of an entire organ, some function supposed to depend upon it still exists, with little or no impairment, we have a right to say that the function *cannot* be dependent upon that organ. On the other hand, the evidence afforded by the disappearance of a certain function or power, after the removal of any particular division of the nervous centres, is less satisfactory, and needs to be sifted with great care. But we are not entitled to reject it as unworthy of consideration, because we find the results in some degree discordant; still less if we find that, after the repetition of such experiments by a considerable number of independent vivisectors, the phenomena themselves present such an accordance, as cannot be accounted for by the general disturbance of the functions of the nervous centres, produced by the severity of the lesion itself.

---

* Lancet, 1845, vol. ii, p. 364.

When the cerebrum of one of the higher vertebrata is entirely removed, the animal is reduced to the level of one of the invertebrata, which is naturally destitute of the organ; and it is important to ascertain, if the actions which it then performs are of the same nature as those, which are so characteristic of the latter. But, in judging of the results of such vivisections, it is important to bear in mind, that the functions of the remaining organs cannot but be impaired by the removal of a large part of the encephalic mass; and also that these functions, in the unmutilated animal, are by no means so obvious in its actions, where a cerebrum is present, as where it is altogether wanting. Thus, whilst nearly all the actions of insects, which are not reflex, may be regarded as the direct results of the exciting sensations, the actions of birds, whilst presenting a general similarity with them, are more obviously influenced by intelligence, and are, therefore, for the most part referrible, not so much to the consensual group as to the volitional. The variability of the actions of different individuals of the same species, under similar circumstances,—the actions themselves all tending to the same end, which is suggested and kept constantly before the mind by the prevailing sensation,—becomes very striking in the class of birds, when we closely scrutinize their habits, and contrast them with those of the other class, to which they bear so remarkable a general analogy. Making such allowances, then, we think that experimental inquiry affords satisfactory evidence of the identity in the condition of a bird or mammal, whose cerebrum has been removed, with that of an insect or other invertebrate animal which naturally possesses none. In the first place, we think it impossible to analyse and compare the results obtained by different vivisectors, without coming to the conclusion that *sensibility*, both of a general and a special kind, remained after the removal of the cerebrum. Doubtless there are fallacies attending the deduction of any such inferences from the movements performed by the animal under various stimuli; since a large proportion of these movements may fairly be set down as of a reflex character, and as not indicating sensation. It is safer, therefore, to draw our conclusions from movements consequent upon such impressions made upon the organs of *special* sense, as cannot (so far as we are aware) excite movements without the intervention of consciousness. And it appears to us to be an inevitable deduction from these phenomena, that the animals retained the possession of the senses of sight and hearing, though in a very imperfect manner. It is not a little curious, that M. Flourens, who at first drew a contrary inference from his experiments,—maintaining that the removal of the cerebrum destroys all sensibility,—should have since abandoned this opinion, in favour of the one put forward by Cuvier and his colleagues, in their well-known report upon M. Flourens' first Memoir; having substituted, in the second edition of his 'Experimental Researches,' the term *perception* for *sensation*, whenever he speaks of the function which is destroyed by the removal of the cerebrum.—Secondly, in animals deprived of the cerebrum, consensual movements were excited by the sensations. Thus, a bird would follow the light, and it would avoid objects placed in its way; it would start from its nocturnal sleep, raising its head and opening its eyes, upon the slightest noise; and it spent a great part of its time in pruning its feathers and scratching itself. Even if we regard the last of these actions as reflex, the preceding can scarcely be so considered. Making a fair allowance, then, for the disturbance in the condition of the whole encephalon, which is necessarily induced by the removal of so important a part, and for the small proportion which the simply consensual actions bear in the general movements of these animals, we think ourselves entitled to affirm, that the conditions essential for these actions remain after the removal of the cerebrum, and that we are not to regard that organ as having any necessary participation in them.

Other experiments prove how much the ordinary movements of birds and the lower mammalia are *directly influenced* by sensation. Thus, vertiginous movements may be induced in pigeons and rabbits, not merely by destroying one of the optic ganglia, but by simply blinding one eye, either by evacuating its humours, or by temporarily bandaging it. And we believe that the similar movements produced, in the curious experiments of M. Flourens, by lesions of the semicircular canals, are readily explicable upon the same principle,—perverted sensations in regard to the direction of sounds (of which it seems to be the special object of that part of the auditory apparatus to take cognizance) being thus communicated to the sensorium, and producing abnormal movements, the direction of which bears a constant relation to that of the canal. These and other similar experimental results, which are referred to by Mr. Noble with a sort of contempt, as not affording us the slightest knowledge of a precise kind in regard to the functions of the different parts of the encephalon, appear to us, on the contrary, when surveyed from the point of view to which we are led by comparative anatomy, to be in admirable harmony with the conclusions indicated by that mode of investigation.

Although the development of the cerebrum in the vertebrata appears to *supersede* the simple and direct actions of the sensory ganglia and the motor fibres proceeding from them,—and this, in a degree comparable to its superiority of size,—it by no means *alters* their function. The simply consensual movements gradually form a smaller and smaller proportion of the whole actions of the animal as we ascend the scale; just as in the invertebrata, we find the simply reflex operations of the lower gradually brought under the guidance and control of the consensual, which are so characteristic of the higher. The very same gradation may be traced in the human infant. No one can watch its actions with an educated physiological eye, without perceiving, that they are at first almost purely reflex; the nipple of its nurse being seized by the lips when they are applied to it, and the act of suction being performed, as Mr. Grainger's experiments prove, under circumstances that forbid any other idea being attached to them. But we soon find that it is guided to the nipple by the sense of sight, and also (we think) by that of smell, which appears to us to have much to do with this action; and the first movements which it performs, when it begins to take notice of surrounding objects, are evidently to be referred to the simply consensual group. This is just what we should expect from the fact, that the spinal cord and the sensory ganglia at its summit are the parts of the nervous centres which are first developed, and which most rapidly advance towards maturity. Their actions are sufficient for the human infant for a long period after its birth; and it is not until it is several months old, that we can discern any, which can be unequivocally attributed to a dawning intelligence. From the time when this manifests itself, the simple consensual movements become less and less obvious; and at last they escape our notice altogether, if we do not carefully scrutinize our own actions. But it does not hence follow, that the sensory ganglia and their motor nerves are thrown out of use; or that their only function is to receive sensations and impart them to the cerebrum, without having any concern in the movements which it originates. For we think that all recent observations and experimental inquiries tend towards this conclusion—that *the cerebrum has itself no direct connexion either with the sensory or motor nerves;* and that as it receives sensations through the medium of the various ganglia, in which the sensory nerves terminate, so it executes the mandates of the will through the motor fibres which originate from those same ganglia, or from others in immediate connexion with them. The belief has long prevailed, that the motor power of the cerebral hemispheres is chiefly exerted through the corpora striata; and we have already proved, from the comparative anatomy of these organs, that they are by no means to be regarded as mere appendages to the hemispheres, but are independent ganglia, of which the cerebrum makes use (so to speak) to perform its movements. Bearing in mind their intimate connexion with the thalami optici, we think it probable (as already remarked), that they constitute the motor portion of the ganglionic centre of the tactile sensations and respondent movements; and, if this be the case, the reason of its peculiar connexion with the cerebrum at once becomes apparent. For all voluntary movements require *the guidance of sensations;* and most of these are of the tactile kind. The "muscular sense" of Sir C. Bell is a necessary condition of our keeping up any voluntary contraction of a muscle, unless some other sense is substituted for it,—as in the case of the woman, affected with anæsthesia, who could hold her child upon her arm as long as she looked at it, but could not, by the strongest effort of her will, sustain it after withdrawing her eyes. In the movements of the eyeballs, the guiding sensations (as Dr. Alison has shown) are ordinarily those received through the sight; and in the movements of the larynx and muscles of articulation, the guiding sensations are ordinarily those received through the hearing; but, in either case, if these senses are wanting, a sufficient amount of guidance seems to be derivable from the muscular sense. No instances occur to us, in which voluntary muscular movements are dependent upon the guiding sensations, derived from the smell or the taste.

Thus, then, the spinal cord with the ganglia at its summit,—forming altogether what may be termed the *cranio-spinal axis,*—to the exclusion of the cerebrum and cerebellum, furnishes all the conditions for the reception of sensations, and the performance of muscular movements; and the two latter organs may be regarded as superadded for some special purpose, receiving their stimulus to action through the medium of the cranio-spinal axis and its nerves, and exerting their influence on the muscular system through the same channel. In adopting this view, which is by no means a novel one, we think that we can harmonize completely all the results afforded by anatomical research and by experimental inquiry. It corresponds particularly well with the varying intimacy of the connexion between the cerebrum and the different ganglia at its base, and with the relative development of the latter. For the connexion is *most* intimate with the thalami optici and corpora striata, which we regard as together constituting the ganglionic centre of tactile and muscular sensations, and of the respondent motions; and these ganglia are in man by far the largest of the whole series; both of which facts seem to correspond with the great predominance of the movements guided by these sensations over all others. Next, in order of intimacy of connexion with the cerebrum, and also of size, come the optic ganglia or tubercula quadrigemina; and this again corresponds with the fact that, next to the tactile sensations, the visual are of most importance in regulating the muscular movements. The auditory ganglia, lodged in the substance of the medulla oblongata, are of smaller size, and have a less intimate connexion with the cerebrum; in accordance, as it would seem, with their inferior concern in the regulation of the voluntary movements. But they are in immediate relation with the central connexions of the nerves exciting the actions of the larynx and of the muscles of articulation; and thus serve the important purpose, as we have already pointed out, of regulating the movements of the organs of speech. And the olfactive ganglia, of which the ganglionic matter in man is present in comparatively small amount, are the most removed, of all the ganglia of special sense, from intimate connexion with the cerebrum; and they are the least concerned in the direction and regulation of the voluntary movements. In regard to the sense of taste, we need only remark that (as most of our readers are probably already aware) there is strong reason for regarding it as only a modification of that of touch; and if it has a proper ganglionic centre, this will be a portion of the vesicular nucleus of the medulla oblongata, in which the sensory division of the fifth pair, and the glosso-pharyngeal nerve find their termination. This position, too, would correspond with the slight participation which the sense of taste possesses in the regulation of the voluntary muscular movements; those of the tongue, in the examination of a sapid body, being the only ones which we can attribute to it.

Before we quit the subject of the sensory ganglia, we must remark that they appear to us to be the seat of the simple or elementary *feelings* of pleasure or pain, which are connected with different sensations. If we deny this, we must deny the existence of such feelings in animals whose encephalon consists entirely of sensory ganglia; which, we imagine, few will be disposed to do in regard to insects, to say nothing of inferior tribes. We shall presently attempt to show that, in animals possessing a cerebrum, these feelings may be associated with *ideas*, the impressions made by which are transmitted from it *downwards* to these ganglia; as well as with the *sensations*, whose impressions are received in an upward direction from the sensory nerves.

We now come to consider the general functions of the cerebrum; in which determination we are guided by subtracting from the sum-total of the functions of the nervous system all that it can be ascertained *not* to do. It would be interesting, if we had space to dwell upon such a topic, to trace the gradual limitation which has taken place in the physiological idea of the functions of this organ; from the period when it was considered as the centre of the whole life of the body,—the elaborator of the "animal spirits," whose circulation through the system by the nerves was regarded as essential to every vital action; to the time of our present writing, in which the real extent of its agency is so much better understood. The withdrawal of the organic functions from its support or control, was the first step; the proof of the independent endowments of the spinal cord was the next; and the persistence of sensibility and of the power of consensual movement, after its removal, was the third great advance in this limitation. The cerebrum, then, has no concern in the purely excito-motor actions. It is *not* the sensorium, or instrument, by which the mind becomes conscious of sensory impressions. Nor does it participate in the performance of the movements, which are directly connected with these sensations. Nor does it seem necessary for the feelings of pain and pleasure, which appear inseparably connected with them. What then is its office? In the first place we may attribute to it the formation of those *notions* or *ideas* respecting the objects of sensation, which are the materials upon which all the higher processes of mind operate; the power concerned in which process, the process itself, and the result of it, are all known by the term perception. We may attribute to it, also, that power of recording sensations and the ideas connected with them, which we call memory; and that power of recalling them, which is designated conception. Now it is easy to understand, that the direct communication between the cerebrum and the sensorium should cause the ideas excited in the former by sensations, to produce feelings of pleasure or pain in the latter, just as do sensations themselves; and the same result will take place from the recall of past sensations and ideas, or the conception of those which are future, founded as this is upon the experience of the past. We lay much stress upon this view, that the *ideas* are *cerebral*, whilst the *feelings* of pleasure or pain connected with them are *sensorial*, for reasons that will presently become apparent. Its correctness seems to us borne out by a variety of considerations. We cannot see any reason for supposing, that there is a different seat of pleasure or pain for a remembered or anticipated sensation, from that in which these feelings are aroused by an actual sensation. The intermediate position of the sensory ganglia, receiving as they do the fibres from the sensory organs on the one hand, and from the cerebrum on the other, points them out to us as the seat of *consciousness*, not only of what is taking place in the *external world*, but also of the *operations of the cerebrum itself;* and if of consciousness, then undoubtedly of the pain or pleasure so immediately connected with it. But we have a stronger ground than this, namely, that the very same consensual movements may be excited by remembered or conceived sensations as by the actual ones; showing the exact analogy between the downward action of the cerebrum, and the upward action of the sensory nerves, in exciting changes in the sensorium. We might cite numerous examples of this kind; the following will suffice. In persons with irritable stomachs, the act of vomiting may be excited by an offensive smell, a nauseous taste, or a disgusting sight; and the remembrance or conception of similar sensations will produce the same effect. When the nervous system is in the condition, that renders the act of yawning easily excitable, it may be induced, not merely by the sight or sound of yawning in another person, but by the mere *idea* of it, called up by the mention of the term. In like manner, in hydrophobia, the *idea* of liquids, called up intentionally or accidentally, is frequently as efficient in exciting convulsive paroxysms, as the actual sight or sound of them.

These considerations appear to us to lead to a view of the nature of the propensities and emotions, which is different from that ordinarily entertained, and which seems to us to harmonize better with our knowledge of mental operations. As we have already noticed, we can only judge of these propensities in the lower animals by the actions they perform; and we are likely to be in great error, if we always attribute to them the same desires or emotions as we ourselves experience, when we execute similar actions. For these desires or emotions involve *an idea* of the objects of them, such as is formed in ourselves for the purpose of stimulating to action the reasoning powers and the will; and in animals which have no reasoning powers, and in which the action is immediately prompted by the sensation, there is no reason to believe that such ideas or notions are formed. Let us take, for example, the sexual propensity. This, in the human being, may take the form of a desire, excited by sensations, some of them internal and some of them derived from without; and this desire stimulates the reasoning powers and the will to the operations necessary for the indulgence, or, it may be, for the restraint of that desire. But in the acts immediately preceding the *emissio seminis* (which is itself reflex) we trace the *direct* operation of sensations; and this appears to be the whole history of the process in those tribes, whose instincts are not restrained or directed by intelligence,—*all* the movements concerned being the direct result of the exciting sensations, in other words consensual. We may easily, of course, believe that the sensations are painful, when sexual intercourse is prevented, as they are pleasurable when it takes place; but we affirm that there can be nothing like a desire, unless there be a capability of forming an idea of the object of that desire, which the sensory ganglia alone are unable to do. Instead, then, of being that simple propensity which some would regard it, the so-called amativeness is of a complex nature. In so far as the action referred to it is simply consensual, the abstract principle (as we have already shown in regard to the propensities in general, p. 504) has no real existence. And in so far as the propensity has the form of a desire, of which the mind is conscious, this desire is resolvable into the formation of an idea or train of ideas, which the mind feels it pleasurable to entertain. The first operation, the formation of the idea, is purely *intellectual;* the second, the feeling of pleasure connected with it, which is the cause of our continually dwelling upon it, and of its powerful operation upon the reasoning processes and will, appears to us evidently *sensorial.* In certain disordered states of the nervous centres, the sexual sensations may be so highly excited, that they act directly upon the muscular system, not only without the aid of the intellect and will, but even in opposition to it, as is seen in the violent semi-convulsive movements, excited in a nympho-maniacal patient by the sight of one of the opposite sex.

In like manner, we might analyse the propensity to imitation; and might show that it is partly sensorial, partly intellectual. Thus there is some imitation, which is altogether consensual; the sensation, in each case, immediately exciting the movement. This is the mode in which infants learn to perform many of the actions which they witness in adults; and in which many adults are led to imitate involuntarily, and even unconsciously to themselves, under circumstances where they would least wish to do so. Take, for example, the case of yawning, before referred to; in which the operation of this consensual power is peculiarly evident, as it is impossible to execute a thoroughly good yawn by an act of volition alone. Now it seems absurd to refer such an action to an abstract principle of imitation, and to attempt to localize this principle; when we can trace a connexion as direct and immediate between the sensation and the muscular movement, as we can between the impression and the respondent action in the case of a movement excited through the spinal cord. On the other hand, there is a propensity to voluntary imitation,* as shown in a fondness for mimicry, &c.; and this, like the amative desire, is resolvable into the intellectual *idea* and the *sensorial pleasure* accompanying it.

In like manner we might analyse all the propensities, emotions, and moral feelings; between which and the intellectual operations, phrenologists would establish such a marked distinction. What is benevolence, for example, but pleasure in the contemplation of the happiness of others? What is the whole class of selfish propensities, on the other hand, but the feeling of pleasure in the entertaining of various ideas connected with self? What is combativeness, but the pleasure of setting one's self in antagonism with others? Or what is veneration, but the pleasure of contemplating rank or perfections superior to our own? What is hope, again, but the pleasure derived from the anticipation of future enjoyment? Or what is cautiousness, but a mixed propensity, resulting from fear or anticipated pain, (to which phrenology does not assign a separate organ,) combined with pleasure in the contemplation of precautions adopted to ward it off? The connexion between the elementary feelings of pleasure and pain, and the operation of the various propensities, is universally admitted; the usual explanation of this fact is, that the indulgence of the propensities is a source of pleasure, whilst the restraint of them is attended with pain. Now, on the other hand, we consider a proper analysis of the processes in question to show, that the propensity cannot exist without a distinct *idea* of the objects to which it is directed; that the existence of such ideas, like that of the sensations from which they originate, is attended with pleasure or pain; and that, according to the respective degrees, in which these feelings attend particular classes of ideas, (as to which there is obviously a difference among individuals, analogous to that which exists with regard to the feelings of pleasure or pain excited immediately by sensations,—the likes and dislikes of different articles of food, for example,) will be the disposition of the mind to entertain them, the frequency with which they are brought before it, and the influence they are allowed to exercise in the formation of our voluntary determinations.

We believe, then, that Dr. Carpenter is perfectly correct in maintaining that the offices of the cerebrum are restricted to *intellectual* operations; understanding, by that term, the operations which are concerned in the formation of a voluntary determination; and the movements consequent upon which are expressly adapted to an end distinctly conceived in the mind of the individual. But this allows full weight to the operations, through the instrumentality of the cerebrum, of what are commonly termed the emotions, as the *active principles* in the guidance of our conduct; since, as just shown, the emotions are not simple but composite states, the formation of ideas being the part of them that is performed by the cerebrum, whilst the sense of pleasure or pain, which is the real *feeling* connected with them, has its seat in the sensory ganglia. And the objection, brought by Mr. Noble and Mr. G. Combe against Dr. Carpenter's views,—that the most active principles of the mind, the mainsprings of the greater number of intellectual operations, are thereby located in a portion of the nervous centres, whose size bears no relation to their importance,—falls to the ground when those views are thus qualified. In the form, in which we have now endeavoured to develope them, they fully explain the mode in which emotions, directly excited by sensations, or recalled by an act of memory, may act upon the nervous system, in such a manner as to produce movements, which themselves belong to the consensual group. We may take the case of laughter as an illustration. The consensual nature of this action is proved by the facility with which it may be excited, in most persons, by the act of tickling; this peculiar impression (conveyed through the ordinary spinal nerves) being competent to excite reflex actions in the part itself, and in other muscles, through the medium of the spinal cord; but only having the power of exciting laughter, when it reaches the sensorium and is *felt*. In like manner the action may be directly excited by sights or sounds, to which no idea is at first attached, and which do not therefore operate by calling up an emotional state. We say that such things are ludicrous, when we come to consider them, simply because they excite involuntary laughter; and we can give no further account of them; and, as in the case of pleasure or pain connected with particular sensations, a sight which provokes one person to laughter may be perfectly inert as regards another. In many cases of this kind, the *will* is strongly exerted to check the consensual movement; successfully or otherwise, as the case may be. The character of the action is further shown by observation of infants, in whom smiles and laughter obviously result from the stimulus of sensations, before they can have any ideas connected with them. But the more ordinary cause of laughter in the adult is a ludicrous *idea*, which is aroused by the sensation; and the seat of this we regard as cerebral. The sensation itself, which was conveyed to the sensory ganglia, was not of a kind to excite the respondent motion, and passed on to the cerebrum without doing so; but the idea excited there reacts upon the sensory ganglia, and produces in them the condition which was excited in the former case by the sensation, and thus calls forth the respondent motion. And the same results will ensue, when the idea has been long stored away in the mind, and recurs involuntarily, or is called up by an exertion of the will. In any such case the presence of the idea, instead of acting upwards upon the reasoning processes, operates downwards upon the body, and discharges itself (so to speak) in an involuntary or emotional action. The emotional states, when they do not thus immediately affect the muscular system, become the subjects of higher mental operations, and influence the determinations of the will, so as to be most important agents in the regulation of the voluntary movements. This double action of the emotions, which was long ago pointed out by Bishop Butler, and which has not been (in our opinion) sufficiently considered either by physiologists or metaphysicians, appears to us to find its best and simplest explanation in the two series of operations which we have attempted to develope,—the one involving only the very simplest cerebral action (the formation of ideas), which at once reacts downwards upon the body through the sensory ganglia, which give to those ideas the feelings connected with them,—whilst in the other, those ideas act upon the higher mental processes, and affect the body only through the will. And there is a well-known fact in our psychological and physiological history, of which this view seems to afford a particularly apposite explanation,—namely, the relief afforded to violent mental emotions, by giving free scope to the bodily manifestations of them. Every one knows the discomfort of suppressing a yawn, how much it adds to our fatigue, and thus makes us increasingly desire the cessation of the discourse to which we are listening. We know, too, how a ludicrous idea continues to haunt us, if we do not let it explode with a hearty "guffaw." And we believe that it is a matter of equally familiar experience on the other side of the Irish Sea, that, when the combative and destructive propensities are strongly excited, they commonly evaporate speedily with the free play of the shillelagh; being much more likely to produce an enduring resentment, which shall call the intellect into exercise for its gratification, if the desired fight be prevented. This we believe to be the true physiology of the relief which some irascible persons find in a hearty d——n, in a violent slamming of the door, or in any other bodily effort to which the emotional condition prompts. The depressing emotions, in like manner, are often worked off with a good fit of crying, sobbing, &c.

---

* In this manner we can understand a fact, of which, though sufficiently common, we never met with a professed phrenologist who could give us an explanation,—that the tendency to involuntary imitation may exist very strongly in persons who are utterly unable to mimic or imitate voluntarily, and *vice versâ.*

We have dwelt long upon these topics, because we are not aware that they have been anywhere handled in a manner at all satisfactory; and having thus endeavoured to limit the functions of the cerebrum to that which we have a fair ground, from anatomical, experimental, and metaphysical evidence, to believe it to perform, we shall briefly inquire how far these views are applicable to the phrenological system as at present held, and what modifications they tend to introduce into it. But it may be desirable to recapitulate briefly the positions upon which we have now dwelt.

1. That the sensory ganglia supply all the conditions requisite for the reception of sensations, in the higher animals as in the lower; and that there is a class of actions excitable through them by the direct influence of sensations; to these we give the name of consensual.

2. That the sensations which excite these actions, also excite the feelings of pleasure and pain, which have their seat in the same ganglia. These feelings may receive different designations, according to the nature of the objects towards which they are displayed. Thus, attachment and dislike, affection and rage, joy and sorrow, and many other simple and elementary feelings, are but modifications or phases of pleasure and pain-

which receive their different designations according to the character of the objects which excite them, the ideas which they arouse, and the mode in which they are manifested.

3. That sensations, the simple feelings connected with them, and the consensual movements to which they prompt, make up the sum-total of those operations to which the term *instinctive* is properly applicable; that these take place through the instrumentality of the *sensory ganglia;* and that none of those higher operations which involve the formation of ideas, reasoning processes, and volitional determinations, can take place without a cerebrum.

4. That the cerebrum is the seat of the formation of *ideas*, or elementary notions originating from sensations, and of all those higher *intellectual operations*, of which those ideas form (as it were) the pabulum.

5. That the occurrence of *ideas* in the cerebrum may produce feelings of pleasure or pain in the sensory ganglia, analogous to those which are produced by *sensations.*

6. That the tendency to the recurrence of a certain class of ideas, constantly connected with feelings of pleasure or pain, constitutes what is known as an emotion, desire, or propensity; and that this is composite in its nature, involving the cerebrum for the formation of the ideas, and the sensory ganglia for the feelings with which they are associated.

7. That certain ideas, which thus strongly excite the feelings, may also produce motions through the instrumentality of the sensory ganglia and their nerves; that these movements are involuntary in their character, and are excited by emotional states in the same manner as they are by direct sensations; and that they consequently belong to the consensual group.

8. That intellectual operations *may* take place, in which the feelings do not participate (as, for example, in mathematical or scientific ratiocination); but that the motives which regulate our personal conduct are, in great part, derived from the feelings attached to particular ideas or classes of ideas. When the emotional states thus act, in affecting the further course of the mental operations, they have no immediate agency upon the body; their influence being exerted through the will.

9. That the exertion of the reasoning powers, and the final determination which, in its action upon the body or the mind, we call volition or will, operates solely through the instrumentality of the cerebrum.

10. That the cerebrum has probably no direct connexion, however, either with the sensory organs or with the muscular system; but that it depends upon the sensorial ganglia for the reception of sensations, and for the execution of voluntary movements; this execution being still guided by the sensations received through these ganglia, and the act of muscular contraction being dependent upon their continuance.

The first and chief point of collision between these views and the ordinary phrenological system, is that which relates to the localization of the passions, emotions, &c.; but it would not, we think, be found difficult to reconcile the two, so far as this question is concerned. For it may freely be admitted that there *are* such classes of ideas as those grouped together under the terms benevolence, combativeness, philoprogenitiveness, or destructiveness; although the pleasure attending the act of entertaining them which causes them to be habitually kept before the view of the mind, and thus gives them the character of propensities, be not seated in the cerebrum, but elsewhere. Now if we find reason to adopt the phrenological system as a whole, the only modification it would require, would be to regard the different divisions of the cerebrum, commonly termed *organs*, in the light of instruments for the formation of the several classes of *ideas*, instead of being the instruments of the emotions or propensities, taken as a whole, into which the ideas so largely enter. And we think that our view further harmonizes well with the fact, that the exercise of *every* faculty, perceptional or intellectual, as well as moral or animal, may be attended with pleasure or pain; and that we may put the intellectual faculties into the form of propensities, just as readily as we do the passions and emotions. For instance, there are individuals who feel a delight in following out a beautifully-connected train of scientific reasoning, which terminates in evolving a simple principle adapted to explain a great variety of complex phenomena,—or in recognizing an analogy between facts apparently remote, which becomes the key to generalization or induction,—fully as keen as that which another will experience in carrying into operation a plan dictated by enthusiastic benevolence, or which will fill the whole nature of a third, in devising some scheme from which he anticipates a large amount of personal benefit. The tendency to exercise the reasoning powers, or to search for these analogies, which result from the pleasure thus experienced, must be admitted to be as much a *propensity* as benevolence or acquisitiveness; and if it be admitted that the former is a complex state, involving the capability of a certain kind of intellectual operation, and the capability of gratification from the exercise of it, we can see no reason why the same method of analysis should not be applied to the other. The different classes of ideas may still be called perceptional, intellectual, moral, and animal, according to the character of the objects to which they relate.

We have no *à priori* objection to make to the doctrine, that these different classes of ideas may be formed by the special instrumentality of different parts of the cerebrum. We freely admit, too, that there is a general correspondence between certain forms of cerebrum, arising from the relative development of its different portions, and certain leading diversities of character; which might not unfairly be regarded as indicating that these several divisions are the special instruments of particular groups of intellectual or moral faculties. We by no means desire to hold back from the inquiry, whether this inference is not a most legitimate one, and whether it may not be further carried out, by the subdivision of these regions amongst the elementary faculties, tendencies, &c., so as to be able to assign to each part of the surface of the cerebrum its special function, and, on the other hand, to point to the instrument of every one of the mental operations. That every act of mind, of whatever character, is accompanied by a *material change* in the nervous centres—the two being so necessarily linked together in our present state of existence as to be altogether inseparable—we most firmly believe; and there is no medium, in our apprehension, between the supposition, that the whole cerebrum acts together in every mental change, however simple, and the idea that different parts of it are appropriated to different elementary processes, and that they are called into distinct or conjoint action, according to the nature of the mental action that is going on. The latter view appears to us to derive support from various anatomical facts, especially from the frequency with which we are enabled to prove, that large ganglionic masses are made up of distinct parts, having really dissimilar functions; and from the gradual increase in the complexity of the cerebral structure, arising from the successive addition of new parts, which we encounter in tracing it from its first appearance at the bottom of the vertebrated scale, up to its highest form in man. Comparative anatomy thus seems to lend us important aid in establishing the fundamental principles, or axioms, of phrenological science; for if the cerebrum were an organ as homogenous in *function*, as it appears to be in *structure*, we should expect that its higher development would consist only in an increase of *size*, instead of involving (as it manifestly does) an increase in the number of its lobes, the formation of convolutions, and a gradual increase in the number of the latter, together with analogous complications of internal arrangement. The doubts which we feel called upon to express, as left in our minds after a careful review of the whole subject, with the aid of Mr. Noble's replies to the various objections and criticisms quoted by him, relate, therefore, not to phrenology or cerebral physiology as a proper object of pursuit, or as a legitimate department of scientific inquiry, but to the reception of the present system, as one founded upon a correct induction of the phenomena which it should comprehend, and as expressing the whole, or nearly the whole, of the truth regarding the psychical nature and operations of man. These doubts we shall class under two heads, as they arise from physiological or from metaphysical considerations.

In the *first* place, the facts of comparative anatomy seem to be very far from coinciding with the *main features* of the system, in regard to the respective functions of the three lobes of the cerebrum; since, as we have already stated, the rudiment of the cerebrum which exists in fishes, and which is still more developed in reptiles and birds, is the representative of the *anterior* lobe alone of the human brain; the middle lobe being first developed in the lower mammalia; and the posterior being not merely restricted to the higher, but being more developed in man, relatively to the remainder of the mass, than it is in any other animal. Now, as upon the usual phrenological allocation, the lower or animal propensities are situated in the posterior lobe, their instrument would seem altogether undeveloped in the beings in which they apparently possess the greatest force; whilst they attain their most complete evolution in that species, which is distinguished by his power of keeping them in subjection. We have already shown that the usual phrenological explanation of this difficulty, confirmed as it is by the history of human embryological development, is altogether incompatible with facts. But we will offer another which may tend to remove the apparent discrepancy. We have already pointed out that we are not always to regard those *actions* of the lower animals, which correspond with our own, as indications of the existence of *propensities* in them corresponding to those from which they emanate in ourselves; these propensities being, in fact, the states intervening between the exciting sensations and the resulting will, and being compounded of ideas and feelings. Now in beings which are altogether destitute of a cerebrum, the actions supposed to proceed from the animal propensities must be really consensual in their nature; and it may be questioned whether they do not remain so, in those vertebrata which have no posterior lobe to their cerebrum; the propensities—that is, the *ideas* of the objects to which they respectively relate, and the *feelings* connected with the mental consciousness or contemplation of them—being really restricted to the higher mammalia, in which they are designed to work upon the intellectual powers and the will, to contrive the means for their gratification. On this hypothesis, we should expect to find the posterior lobe attaining by far its highest development in man; since, in the well-regulated mind, the animal tendencies never act otherwise than through the reason and will—that is, in the form of true propensities. And this *gradual* removal of the actions in question, first from the domain of reflex into that of consensual action, and then fromt he consensual or instinctive to the volitional group, is precisely conformable to that which we find in regard to other operations of the nervous system—the act of progression, for example. We trust that our views may find more favour among our phrenological readers, from their affording them a feasible method of extricating themselves from this very awkward dilemma, without abandoning any of their fundamental positions.

Secondly, in so far as comparative anatomy has been appealed to in support of the details of the present system of phrenology, the deductions drawn from it are entitled to but little weight, for the following reasons. A large part of these deductions was made prior to our present improved acquaintance with the functions of the nervous centres, and the real character of many of the actions of the lower animals was greatly misunderstood. In saying this we would not be thought to do injustice to Gall; to whom we are under great obligations for the advances which he made in comparative psychology, and in the comparative physiology of the nervous system. We are not unmindful of the fact that he was among the first, perhaps the very first, to advocate the doctrine that the ventral ganglionic cord of articulata is the analogue of the spinal cord of vertebrata. Nor do we forget how much he contributed to the establishment of these views, in regard to the combined action of the instincts and of the reasoning faculty among the lower animals, which are now current among well-judging psychologists. But it cannot be forgotten that the whole doctrine of reflex action, and of the dependence of the greater part of the actions of invertebrata, with a considerable proportion of those of vertebrata, upon the spinal cord alone, has been established since his time; that the identification of the cephalic ganglia of invertebrata with the sensory ganglia of vertebrata has only been recently accomplished; that the reference of a large class of movements to these bodies, independently of the cerebrum, being consequent upon that identification, is only now claiming attention as a physiological truth; that the actions of which the cerebrum is the instrument, have, consequently, not yet been clearly distinguished from those in which it has no direct participation; and that analogous *actions* may be referrible to entirely different *springs of action*, when we contrast one species with another, as when we compare the motives which operate in different individuals of the human race; so that we are not to nfer the existence of a particular psychical propensity from the observation of a series of habitual actions, which may be really consensual. Further, in reference to the mere anatomical comparison, we must remark that enough pains has not been taken to bring together facts which are really similar. We hold all comparison of *crania of different species* of animals to be utterly worthless as furnishing materials for phrenological deductions. It affords no key whatever to the comparative development of different portions of the cerebrum, as is shown by the fact that *similar* projections on the cranium, even in closely-allied species, are found to have under them *different* convolutions of the hemispheres; and by the total inability of this kind of examination to lead us to an exact knowledge of the relative development of even the different lobes of the cerebrum, as is evident from the circumstance that there is no external distinction between the middle and posterior lobes, and that even the distinction between the anterior and middle may not be apparent in the cranium. Again, the external contour of the cerebral hemispheres, in all cases in which they bear a small proportion to the masses beneath them, will be greatly influenced by the form and size of those masses—a circumstance which has been totally overlooked in comparative measurements of the cerebra of different animals, both by phrenologists and their opponents. Moreover, we believe that the general form of the cranium, in different species of animals, is greatly modified by its instrumentality in other functions, especially in mastication, and by its position upon the trunk and mode of muscular connexion with it; and that the disposition of the several component parts of the encephalon may vary with the general form of the cranial cavity, so that no valid inferences can be drawn from the latter as to their relative size. All observations made upon crania of different animals, therefore, ought, we think, to be put aside; and those data alone to be received which are derived from careful examination of the cerebrum itself. And with respect to nearly all the recorded observations upon the conformation of the hemispheres, we may remark that they are open to objection, on the ground of a want of sufficient acquaintance with their anatomical structure, to warrant us in receiving all those phenomena as really analogous which profess to be so. The only true rule—the applicability of which to the nervous system becomes more apparent, the more we become acquainted with the fundamental nature of its operations—is to compare *like* things with *like.* Now the cerebrum of a bird is *not* like that of a mammal as a whole, but can be compared only to its anterior lobe; and it seems absurd, therefore, to be looking there for the organs of combativeness, philoprogenitiveness, destructiveness, or secretiveness, which belong to the posterior and middle lobes. What, then, becomes of all such organs? We reply that it is much more easy to conceive of their having no existence in the bird's mind as propensities, the actions referred to them being simply consensual, than it is to suppose that the uniformity which we every where see in nature is so egregiously violated. The only safe guide in this department of comparative anatomy seems to us to be to follow out the divisions which nature indicates to us—namely, the cerebral convolutions—in all animals in which they exist; and to devote our first, or even (for a time) our sole, attention to these; at the same time, endeavouring to analyse the respective characters of the different animals, with especial regard to the objects upon which their intellectual faculties are employed, so as to distinguish between the merely consensual actions and those which result from the operation of the will guided by psychical propensities. Until all the knowledge has been acquired that this kind of concurrent investigation is capable of yielding, we think it altogether unprofitable to attempt to investigate the cerebral physiology of animals on whose hemispheres there are no convolutions. We shall not stop to discuss here how far the respective observations of Gall, Vimont, Tiedemann, Leuret, and others, are valuable for the end we have in view; our conviction being, for the reasons already stated, that the whole subject must be investigated anew; and the figures and descriptions of all the authors we have referred to being far from sufficient, in our estimation, as the foundation of such an investigation.

Thirdly. We cannot feel at all satisfied, that the method of determination hitherto employed, as regards the human cerebrum alone and its functions is sufficiently trustworthy. The greater part of the observations upon which the details of the present system of phrenology rest have been made upon crania alone, or upon casts, which are still more fallacious. Now although we do not charge this method of observation with being as open to objection, in comparing different individuals of the same species, as it is in comparing different species with each other, yet still we think, that its results are likely to be erroneous and imperfect; for the following reasons. There appear to be, in different races of men, *typical forms* of skull, the general contour having little or no relation with the comparative development of different parts of the encephalic contents, but simply modifying their disposition in the cranial cavity. This assertion we make upon the authority of Professor Retzius, who has studied this department of ethnology more profoundly than even Dr. Prichard himself, and who has shown that the classification of nations, formed according to the shape of the cranium, has so little correspondence with that which would be based upon their correspondence or diversity of psychical characters, as to indicate that no inferences can be drawn from one class of facts in regard to the other. We do not think that phrenologists can hesitate in admitting the possibility of two brains, possessing the same relative development of their respective parts, being moulded into two different shapes, according to the form of cranium peculiar to each race; since they have always admitted the possibility of much more extensive displacements as results of disease. But further—and this is, perhaps, our most serious class of objections to the *present system* of phrenology—no external examination of the cranium, such as can alone be made on the living head or the unopened skull, can give any account of the form of the *base* of the hemispheres, which, being covered with convolutions equally with any other part, must be supposed to have a like participation in the cerebral functions. And even when the examination is made upon the cerebrum itself, the large surface of each hemisphere in apposition with the falx is altogether neglected. Further, as the inner surface of each hemisphere is always flattened against its fellow, no increased development of any part of it can manifest itself, except by thrusting out a corresponding portion of the external side of the hemispheres; so that a protuberance of a certain portion of the latter may be due to a cause altogether irrespective of increased development of the organ to which it seems to belong. Now from the difficulty of determining the development of the basal portion of the cerebrum, and from the absolute impossibility (in the present state of our anatomical knowledge) of judging, even from the cerebrum itself, of the development of the internal portion of each hemisphere, it has happened that scarcely any attempt has been made to map out the *base* of the brain into organs, and no attempt whatever has been made to show what share is taken by the internal surface of the hemispheres in the psychical operations of man or any other animal; so that we are scarcely beyond the mark in asserting that nearly (if not quite) *one half* of the cerebral surface is *totally unappropriated.* Yet not the slightest ground can be adduced for the supposition that these unappropriated portions have any less participation in the operations of the intellect, the exercise of the moral feelings, or the influence of the animal propensities, than have the external and superior portions of the respective lobes. We have met with some phrenologists who were candid enough to admit this to be a difficulty of which they could not give any explanation; their belief being that further observation would lead to increased knowledge of the subject. But in no system of phrenology that we have consulted have we met with an admission on this point, at all adequate to the extent of the difficulty; for, be it observed, one of the great claims which is set up in behalf of the phrenological system is the completeness of the system of psychical philosophy which it presents; the whole series of mental phenomena being arranged under certain categories, each of which has its corresponding representative on the cerebrum. Now if this be the real state of the subject, what are we to believe as to the geography of the terra incognita not yet explored by phrenologists? Are there no "organs" in it? If there be, for what sort are we to look? And if there be not, how can their absence be explained?

The fourth and last of our sources of doubt, in regard to the physiology of the present system of phrenology, arises from the want of sufficient accuracy in those observations upon the human subject upon which it professes to be founded; and from the prevalence of the habit of attending to and recording the *coincidences* between certain cerebral developments and psychical manifestations, without due regard to the cases in which there is *no* coincidence. The difficulty of precisely recognizing the relative sizes of different organs from external examination of the head has often been acknowledged and regretted by phrenologists themselves. It leads to the formation of very different estimates of character from the same data, as we can testify from our personal experience; three extremely diverse accounts having been given of our own "developments," by three well-known phrenologists, in the course of a few months. In another case, which happened to no less an authority than Spurzheim, two very different characters were given of the same individual (a young lady) within half an hour, under the following remarkable circumstances. Dr. Spurzheim was requested to examine the heads of two young ladies, sisters, who so closely resembled each other in person that the wearing of a cap by one of them was necessary to enable even their parents to distinguish them, and who yet differed considerably in psychical character. The capless young lady having undergone the doctor's manipulation, left the room, put on her sister's cap, and returned for a second scrutiny, which was made under the impression that the other sister had appeared; and an extremely different statement of her character was then given. Now either Dr. Spurzheim *intentionally* varied his account, to meet what he believed to be the difference in character between the sisters; or he was unconsciously influenced by the expectation of finding a difference in the developments, the want of any accurate method of measurement causing him to make an involuntary error in his estimate. Our readers may adopt which explanation they please; but the fact, which we have heard upon the most excellent authority, indicates (however explained) that the absence of such a method, by which fallacies of either kind could be detected, invalidates all the observations of Dr. Spurzheim, and leads us to the suspicion that, in other cases also, the process of taking the developments is open to serious error. And further, there is an equal, if not a greater difficulty, in a large proportion of cases, in the acquirement of such a correct knowledge of the character of the individual, and of the *hidden springs* of his actions, (which are often very different from those which would be supposed to be in operation, even by his most intimate relatives and friends,) as shall enable us to ascertain with certainty whether a coincidence exists or not. Every one knows how readily, in phrenological examinations, as in mesmeric performances, such coincidences are caught at; and we are satisfied, from instances which have fallen under our own observation, that a certain coincidence of the *conduct* with the line marked out by phrenological prediction, may result from a *character* very different from that, which the cranium was supposed to indicate, and which gave no indication of actions and feelings known only to the individual himself. All observation, then, which has reference to points of character, about which there can be any doubt or mistake, is valueless for establishing any organological system; and the only valid indications are to be derived from the observations of individuals, in whom the manifestation, or the contrary, of particular faculties or propensities is so decided, that there can be no reasonable hesitation as to their existence to an unusual degree, or their deficiency. We must do Gall the justice to remark, that this was the method adopted by him in his investigations; and if his followers had restricted themselves to the same practice, until a determination of organs had been thus effected, which should be generally satisfactory to phrenologists and metaphysicians, we believe that phrenology would be at present in a state of more real, though less apparent advancement.

But all observations, however carefully made, however sagaciously interpreted, are utterly valueless, unless the failures are chronicled equally with the successes. Will any professed phrenologist, of known candor, come forwards, and say that he has done so, and that he has given his *entire* experience to the public? We do not address our query to those whose minds are so engrossed by the spirit of system, as to consider their science so perfect and unassailable, that no objections *could* be brought against it, which they would be unable to explain away. Nor do we desire an answer from those who have long considered the propagation of phrenology the sole business of their lives, and whose notoriety entirely depends upon their advocacy of it. But we put the question, in thorough good feeling, to those who have become persuaded of the truth of the present phrenological system, after what they deem a sufficient course of inquiry; who can say in singleness of heart that they have no personal motive for upholding it; and who are competent, from their habits of observation, from their knowledge of human character, and from their acquaintance with their own mental constitution, to judge when there is a real coincidence and when there is not. We lay great stress upon this last qualification. We all know more or less from our own experience the truth of the saying "we easily believe what we wish," and unless we carefully watch ourselves we are continually being influenced, in our scientific observations and deductions, by our feelings regarding the question upon which these bear. This influence may be exerted quite unconsciously, and therefore innocently, on the part of the individual himself, though it is frequently obvious enough to others; and we ought, therefore, to be doubly on our guard against it. We think we perceive many traces of it, for instance, in the present work; though no man is more capable than our author of pointing out a similar fallacy in the pretensions of the mesmeric system, or of detecting the sophistry that lurks in all accounts of wonderful cures resulting from the employment of a particular remedy or method of treatment. We believe that phrenologists, like mesmerists, and too many doctors, are continually subject to this kind of influence, which has the more scope for its exercise, the greater is the difficulty of making an *exact* system of observations. The chemist who can weigh his reagents with the greatest nicety, or the astronomer who can measure the movements of the heavenly bodies to a marvellous degree of accuracy, is far less subject to errors of this class; yet neither is exempt from it in the process of forming deductions from such observations, as a survey of the history and present state of chemical and physical science would most abundantly show. In phrenology this source of error is liable to operate alike in cranioscopical observations, in the process of deduction from it as to the probable character, and in the estimation of the real character; and it needs but a very little acquaintance with the doctrine of chances to be aware that the introduction of even a slight element of uncertainty into three distinct processes bearing upon one another, most seriously impairs the probability of correctness in the final result. We could not, therefore, by any means trust to the coincidences, even if honestly recorded with *all* the failures; unless we knew the character and qualifications of the party by whom the observations were made. And our present estimate of the phrenological mind is not sufficiently favorable to lead us to accept the published records of the coincidences as a fair expression of the entire evidence on the subject. For example, we know of an instance in which Dr. Spurzheim pronounced the organ of number to be *deficient* in an individual who was at that time known as the "calculating boy," and who is now an engineer distinguished for his readiness at computation; and we have known the *absence* of the organ of colour to be stated by an eminent London phrenologist as the only remarkable point about the head of a man who was possessed of such powers as a modeller as to be able to produce an exact coloured representation, by the aid of memory alone, of any object to which his attention had been directed. We have heard of many such failures, but they do not find any record in phrenological publications. And the reason why they are not duly chronicled by the opponents of the system, and set in array against the imposing results of Mr. A.'s visit to a prison, Dr. B.'s to a madhouse, and Mr. C.'s to a school or factory, is simply because no one has thought it worth while to accompany Mr. A., Dr. B., and Mr. C., upon their various perambulations, and to note the results throughout. We do not mean to assert that the failures *would be* so numerous as to invalidate the positive results, when both are fairly collected and compared; but we do assert that, *until* such a collation has been made, no more weight is to be attached to the records of positive or coincident observations, than we should admit to the marvels of clairvoyance, or to the accounts of the cure of consumption by a particular remedy, both which are detailed to us by numerous witnesses, who would not knowingly propagate an untruth. There is one more fallacy, which we would point out as resulting from the want of sufficiently accurate means of measuring craniological developments, and from the facility with which the mind of the observer is influenced by extraneous considerations; and this is, the information conveyed by the expression of countenance and general deportment, which, even when not a word is spoken, is often sufficient to afford a fair general knowledge of the character of the individual. We believe that many of the collections of observations, which seem to indicate the triumphant success of the present system of phrenology, especially those made in prisons and lunatic asylums, might be made with nearly, if not quite, equal success, by a good physiognomist; and until a fresh and impartial collection of such observations shall have been made, in which this source of fallacy shall have been excluded by the interposition of a mask, or by some similar means, we cannot feel that they ought to be relied upon as contributing, in any important degree, to the support of the existing phrenological system.

It is against *this system*, and not against phrenology, or cerebral physiology as a legitimate subject of investigation, that we have felt called upon to adduce the foregoing objections, derived from the want of correspondence between its dogmas and the unquestionable facts afforded by comparative anatomy, and from the fallacious and insufficient character of the evidence on which it mainly rests. And we believe that it will share the fate of other systems, religious, ethical, and physical, which have been upreared with similar haste and upon a like insecure foundation; being destined, if not to be undermined and overthrown by more persevering and well-directed inquiry, at any rate to be reconstructed on a plan which shall embrace wider foundations, and be followed up with more securely-laid courses. It is no disgrace to the early framers of the system that it should now require such extensive modification; for the whole science of physiology has been remodelled within the same period, and the almost utter ignorance which prevailed when they pursued their labours, in regard to the special functions of the nervous system, has been in great part cleared away. That the Gall-and-Spurzheim system of phrenology should require, therefore, something more than the slight additions and changes it has undergone at the hands of Mr. Combe and his coadjutors, to reconcile it with the present comparatively advanced state of our knowledge of comparative anatomy and of general neurology, cannot be deemed a bold assertion. Nor should those who volunteer their aid in this reconstruction,—by bringing as materials their stores of knowledge, acquired by modes of investigation totally neglected by the professed followers of Gall and Spurzheim, and by offering to unite their labours with those of the latter, if they can only be assured that a fair use will be made of their work, and that it will not be thrust aside for contributions of really inferior value, but presenting a more specious appearance,—be treated as opponents and destructives. We have endeavoured to earn for ourselves the credit of directing the public mind in the estimation of the present claims of mesmerism; and more recently we have ventured to question the time-honoured systems which at present constitute what is known as medical science. In both cases we have endeavoured to separate the specious from the real, to show how one doctrine after another, that at first commands the general assent of those to whom it is presented, is scattered to the winds when its pretensions are fully and fairly analysed, (a few grains of corn being perhaps left, after a whole bushel of chaff has been thus dissipated,) and to indicate the methods which will be more likely, in our estimation, to evolve solid and substantial truth. We have been endeavouring to do as much for phrenology, and would claim, on that account, to be regarded as the real friends of the science *as it may be*, in spite of the opposition of the professed advocates of the *science as it is*, for which we are fully prepared.

In order that we may put our readers in possession, so far as possible, of the whole state of the question, we must ask their attention to certain metaphysical difficulties, or rather deficiencies, attendant on the present system of phrenology. We should not think it necessary to point out these, were it not for the claims which phrenologists have set up, as the expounders of a new system of psychical philosophy, far more perfect than any which had been offered to the world from any other quarter; capable of explaining all the diversities and contrarieties of human character, and therefore better adapted than any system that can be formed independently of phrenology, to be the guide in education, in the treatment of criminals, and in many other momentous decisions which deeply involves the welfare of the human race. Such claims can only be set up by persons of very shallow acquaintance with the conclusions formed by intelligent observers of human character (to say nothing of professed metaphysicians, who are not always the best guides in such matters), antecedently to phrenology, or independently of it. We have known an individual, for example, who had been practically and most successfully engaged in education for upwards of forty years, assured by a leading professor of the phrenological system of education, that the "new philosophy" would afford him vast assistance in his vocation; but after sedulously attending the course of lectures, in which this system was expounded, our informant declared that he had not heard a single principle enunciated which had not been constantly in his view, from a time when the claims of phrenology were unknown in this country. We believe that it would not be difficult to show, too, that the advances made within the last thirty years towards a better understanding of the nature of mental derangement, and the improvements in the treatment of that condition, are due fully as much to the general progress of inquiry upon these subjects, as they are to purely phrenological investigation. But we are quite ready to admit, on the other hand, that the writings of phrenologists have done much to accelerate that progress, especially by fixing attention upon the *propensities* as the chief springs of human action, and by bringing into prominent view the fundamental diversities in the characters of different individuals, resulting from original differences in the relative force of those propensities. And we are quite ready to give full credit to Gall, as the first (we believe) to enunciate clearly the true relations between the psychological character of man and that of the lower animals, by pointing out that, in the former, as in the latter, instincts are at work, as incitements to action; and that, in many of the latter, as in the former, these instincts are placed under the guidance of reasoning powers, more or less developed. But all these views may be and are entertained quite independently of the organology of phrenology, and cannot therefore be said to be in any way peculiar to the upholders of that science.

With regard to the merits and deficiences of the purely psychological portion of the phrenological system, we cannot here enter upon them at any length. For a very able, though brief, examination of them, we may refer our readers to a valuable and impartial work,* lately published by a gentleman, whose extensive acquaintance with the existing systems of psychology, and whose unprejudiced judgment, render him peculiarly capable of pronouncing an opinion on this topic. That he is by no means insensible to the benefits which science has derived from phrenological investigation, appears from the following conclusion to his preliminary remarks upon the subject: "It has, in a word, thrown a light upon our knowledge generally as to the functions of the encephalon, which did not exist before, and so far has conferred a benefit upon the science of man, which it were uncandid not to acknowledge." "But with these, its physiological researches," he continues, "as it appears to us, the whole of its advantages terminate." It is clearly shown by the author to whom we refer, that no organological comparison can suffice to evolve a system of psychology. "If every organ had its name and nature inscribed upon it by the Creator, then we should have a system of psychology at once; but, so long as this is not the case, we must observe and classify our mental phenomena by reflection before we can begin to map out the locality in which they are to be found." "Strictly speaking," he continues, "phrenology cannot reveal a single intellectual fact which was not equally known before; it cannot trace any points of human knowledge to their primary elements; it cannot perform in any case a single analysis of our complex notions; in a word, it can do nothing, allowing its facts to be true, but point out a certain connexion between two parallel series of mental and physical phenomena." If it be urged that the very circumstance of different feelings or faculties operating in connexion with certain portions of the brain is a clue to a correct classification of them, it may be fairly replied, with Mr. Morell, that "we did not require any phrenological aid to convince us that the animal passions, the moral feelings, and the intellect, present three different classes of phenomena, which cannot be perfectly resolved into each other." And in regard to the more detailed allocation, if we take into account the uncertainty and indefiniteness of phrenological investigation, and the admitted sources of error attending its determinations, even if the fundamental principles on which they rest are conceded, we may well doubt "whether the slightest aid could ever be afforded by phrenology in analysing our mental phenomena;" nor can we yet believe "that a classification grounded upon the positions of the organs can be in any way so satisfactory as one which is grounded upon an accurate observation of the mental phenomena themselves." In fine, with regard to some of the most important problems of metaphysics and morals, phrenology has never attempted a solution, and cannot afford the slightest aid in the search for it. "The only thing it attempts is to ridicule the questions themselves; which is a method of treating them alike easy and ignoble." After glancing at some of these questions, Mr. Morell thus concludes: "We repeat, therefore, that phrenology ought to have taken its place as one branch of physiological investigation; that viewed in such a character it has succeeded in educing many interesting and valuable facts respecting the material changes which accompany the exercise of thought and feeling; but that, in attempting to take its stand as a system of intellectual philosophy, it has entirely mistaken its proper place, and totally failed in throwing any light whatever upon moral or metaphysical researches."*

---

* History of Modern Philosophy. By the Rev. J. D. Morell, A.M. 1846.

Only one portion of the encephalic mass now remains for our consideration, namely, the cerebellum. The anatomical distinctness of this from the other nervous centres is so marked that no physiologist entertains a doubt as to its speciality of function. With regard to the general nature of that function, there is an almost universal agreement amongst those physiologists who attach most weight to comparative anatomy and to experiment; whilst, on the other hand, an entirely different function is attributed to it by those who place the greatest confidence in observations upon man alone. In regard to the nature of the evidence supplied by comparative anatomy, we shall here only observe *in limine* that it affords us the means of readily contrasting the extreme states of development of the organ within the limits of the vertebrated series; for we may trace its gradual diminution, as we descend the scale, through the higher mammalia, the lower mammalia, birds, reptiles, and fishes, until we lose it altogether in the lowest of the latter class. Such a comparison we must regard as more likely to evolve the truth in regard to its function than any collection of observations upon the human cerebellum, in which the size and weight of the organ have not been most rigorously determined. Mere craniological observations we must regard as totally valueless, at any rate until a much more complete knowledge has been attained as to the influence of particular types of conformation of the skull; for the observations of Professor Retzius upon the varieties of form which the cranium presents in different races have indicated this among other facts,—that the *position* of the cerebellum may vary considerably, being much more horizontal in one case and more vertical in another, so as to correspond with a greater or less posterior protuberance, without any corresponding variation in the size of the organ itself. And we shall presently see that Gall was misled by his craniological observations, in estimating the influence of castration upon the condition of the cerebellum.

---

* Op. cit., vol. i, pp. 411-26.

In regard to experiment, again, this organ affords peculiar facilities. It can be removed, partially or completely, without any very severe lesion of the remainder of the encephalon; and animals may long survive the operation, and indicate the results of the ablation of the organ, after the immediate effects of the injury have passed off. We do not hesitate to affirm that there is as great an agreement between the actual *results* of experiment upon the cerebellum, as there is in regard to those of any other class of physiological experiments, involving numerous precautions, and attended with peculiar sources of fallacy; although the *inferences* deduced from these results have varied, according to the preconceived notions of the respective experimenters. Of these variations the closest followers of Gall have not been slow to avail themselves, by way of excuse for putting aside the experiments altogether, as totally valueless for yielding any results that can at all be depended on; yet there have been not a few of the upholders of his general system, who have admitted the force of the evidence, and have adopted those deductions from it which are now received by most phrenologists, without finding it necessary to give up the doctrine of Gall regarding the connexion of this organ with sexual desire. Mr. Noble, however, is not one of these; and we must own ourselves completely at a loss to understand how his usual acuteness of perception should have so unaccountably deserted him, as it seems to us to have done, in the analysis and contrast of the experiments of Rolando, Flourens, Magendie, and others, into which he enters near the commencement of his book. The only solution, in fact, that we can offer of his want of perception of the fundamental accordance of *all* these experiments, is that he has been so blinded by his preconceived views, as to be able to perceive only the points of difference, which exist, as we shall presently show, rather in the minds of the several interpreters of the experiments than in the results themselves. Thus he says:

> "It will thus be seen that no two of the above instances presented anything like coincidence in the results; but that, on the contrary, direct contradictions occur. Rolando's paralysis is met by Bouillaud's no paralysis; Flouren's *inability* to *regulate* movement is counterpoised by Magendie's *capability*, confirmed by Fodera's experience; and the some contradiction is seen throughout the entire history of these vivisections. There is no single fact recorded by one operator which is not counteracted in its tendency to any conclusion by the experience of some of the others." (p. 25.)

The ordinary physiological view of the functions of the cerebellum, founded upon the experiments above referred to, and confirmed by numerous others, as those of Hertwig, Budge, and Longet, which Mr. Noble does *not* cite, is that the cerebellum is the organ for combining and reregulating *voluntary* muscular actions, in which the united action of many muscles is required; especially those concerned in locomotion, and in the maintenance of the equilibrum of the body. This harmonizes well with the fact, that *no cerebellum exists where there is no cerebrum;* and that there is a pretty general conformity between the relative development of the two organs. Where the movements are purely *consensual*, still less when they are *reflex*, no cerebellum can be required to regulate them; and we therefore are surprised that Mr. Noble should have quoted (p. 255,) the regular movements executed by the decapitated trunks of insects, as having any bearing on the question. He surely does not mean to affirm, that in a man, whose spinal cord has been divided in the dorsal region, and whose limbs execute the most powerful reflex movements, any kind of stimulation will cause these limbs to execute the combined movements requisite for the maintenance of his body in the erect position, still less, to perform the act of progression. If this cannot be effected, the balancing and co-ordinating power must have its seat in the encephalon of those animals, whose movements are not purely reflex. We see abundant proof that, in the invertebrata, the encephalic power over the movements of the body is limited to the control and direction of the actions, which, in themselves, are individually reflex; thus, in one of Mr. Newport's experiments upon a centipede, the removal of the ventral cord from the middle of the trunk caused the complete paralysis of the members immediately connected with the abstracted ganglia, whilst the posterior members continued to move and to force the body onwards, in harmony with the members anterior to the part of the cord removed, so long as the latter were in action; but when the former *ceased*, in consequence of an obstacle placed before the animal, of which it evidently took cognizance through the medium of its eyes, the latter *continued*, in consequence of their removal from the control of the cephalic ganglia. We have no reason to believe, then, that the purely *consensual* actions, however complicated, require such an instrument for the combination of the muscular movements needed to execute them; since the complete absence of the cerebellum in invertebrata shows that all which the sensory ganglia effect may be accomplished without it. We may, therefore, infer with certainty, that all the consensual movements in man and the higher animals,—such as sneezing, laughing, vomiting, and the like—are independent of this organ, the muscles being called into combined action by the direct influence of the sensations. But a little consideration will show, that movements executed in obedience to the will, of which the cerebrum is the instrument, are likely to need some such special organ for their regulation; for the will seldom exerts itself *directly* upon the muscular system, if, indeed, it ever does so at all, —its power being restricted to the issuing a general mandate (so to speak) for the execution of a certain action, of which the intellect has conceived the idea; and the selection and co-ordination of the various instruments, by which that action is to be accomplished, being effected altogether without any power on our own parts to direct it. We doubt whether we can will to put *any* muscle into action, independently of the rest; our power being limited to the execution of a particular movement, in which we know that muscle to participate, but in which its action is accompanied by that of others, from which we cannot isolate it. The only cases in which we *appear* to have such a power over individual muscles, are those in which the movements are so simple as to require only the single action of those muscles for their performance.

It appears to us perfectly conformable, therefore, with our general knowledge of the relative operations of the encephalic centres, to suppose that an organ should exist for co-ordinating the movements of the different muscles, in the various actions, most of them possessing a considerable degree of complexity, which are performed by the centres of the motor system of nerves, in respondence to the mandates of the will. And the anatomical connexions of the cerebellum with the sensory ganglia, through which the sensations are conveyed that *guide* the individual movements, and with the medulla oblongata, through which the motor impulses are conveyed to the nerves, harmonize well with such a supposition. The peculiar connexion of the cerebellum with the sensory ganglia was supposed by Foville to indicate that it is the actual seat of sensation; an idea at once negatived by the entire absence of the organ in invertebrated animals. We shall now proceed to inquire how far the above doctrine is borne out by the results of experiment, and how far the discrepancies pointed out by Mr. Noble should lead us to reject those results as valueless. We would first remark, however, that we must carefully bear in mind the difference between the results of the simple ablation of the organ, and the direct results of the injury which that ablation involves. All convulsive movements fall under the latter category; for, as will presently be shown, they subside as the shock and irritation pass off; whilst we are to look in the residual phenomena for the simple results of the loss of the organ. How much may be learned from the latter, after making all necessary allowance for the former, will appear, we think, from the following analysis.

In the experiments of Rolando, made in the infancy of our knowledge of the physiology of the nervous centres, the severe injury or removal of the cerebellum caused inability on the part of the animal to sustain itself upon its legs, or to execute any movements of progression; the animals experimented on soon died, however, with convulsive spasms and hemorrhage. Rolando was *previously* possessed with the view, that the cerebellum is the sole source of the muscular movements, and he represents the ablation of the cerebellum as producing hemiplegia or complete paralysis (according as one or both hemispheres were removed), disregarding the fact which he records, that movements both of the anterior and posterior extremities, *not* of a convulsive nature, were observed after the operation. These movements he attributed to the mere mobility of the muscular fibre, or to some remnant of the cerebellum! We may safely affirm, then, that these results are in no respect contrary to those of Flourens and others, in which no paralysis was indicated. Between the results of the experiments of Flourens, Bouillaud, Budge, and Longet, there is a complete coincidence in regard to the essential fact, that a mammal or bird, deprived of its cerebellum, cannot maintain its equilibrium or execute any regular movements of progression, although it can still move its limbs with energy in all directions. And parallel experiments upon the removal of the cerebral hemispheres fully prove, that the severity of the operation is itself not the cause of the result, since this much more severe operation does not produce it, animals being able to stand, walk, fly, &c., as well after the removal of the cerebrum as when it is entire. "Take two pigeons," says Longet, "and remove from one of them the whole of the cerebral hemispheres, and from the other only one lobe of the cerebellum; the next day the first will be firm upon his feet, whilst the second will exhibit the unsteady and uncertain gait of drunkenness." But, it is urged by Mr. Noble and other adherents of Gall's views, all these results are in direct opposition to those of Magendie and Fodera; in which the effects of the removal of the cerebellum appeared to be confined to the imparting of a tendency to a peculiar *direction* of movement, the power of executing combined movements being asserted still to exist. Magendie found that division of one of the crura cerebelli caused the animal to continue rolling over towards the corresponding side, and that deep wounds of the organ occasioned a tendency to retrograde movement. even when the animal made an effort to advance. These disorders may continue for many days, or even weeks. In the experiments of Fodera, there was an evident disturbance of the motor function, but no constant result. The animals seem to have all died from hemorrhage within a very short period after the operation. Now, these results are readily explicable on the idea, that the sensory ganglia are the proper centres of all movement which is not reflex, for we find that any disturbance in the condition of the latter, or of the *guiding* nerves connected with them, may produce the same result. We have already referred to the tendency to move in particular directions, as a consequence of section of the nerves of the semicircular canals, of the destruction of one eye, and even of a temporary obscuration of vision; the same result follows deep wounds reaching to these bodies; and it is not surprising, therefore, that lacerations of the commissural fibres connecting them with the cerebellum should produce a similar effect. Now that the irritation of these fibres is the cause of the peculiar movements in question, is shown by the experiments of Budge, who found that the movements would spontaneously subside after a time, but that they might be re-excited by irritating the cut surfaces of the crura cerebelli. With regard to the regular combined movements, said to have been observed by Magendie, we find none particularized but this,—that, on vinegar being held under the nose of a guinea-pig, whose cerebellum and cerebrum had been removed, it sneezed and rubbed its nose with its paws. These actions are evidently either consensual or purely reflex; and we might just as well quote the respiratory movements, or the efforts made by a decapitated frog to push away the probe that irritates its cloaca, or to grasp the female, as adduce the sneezing of a guinea-pig and the rubbing of its nose, when an irritating odour was applied, in proof of the power of co-ordinating the muscles for *voluntary* movements. In no record of experiments upon the cerebellum, save in those of Fodera, do we find that the capability of sustaining the equilibrium, or of executing combined and regular movements of progression, remained after the removal of the organ. The experiments of Fodera, as narrated by Gall, seem to have given but very imperfect results, partly in consequence of the very early death of the animals which were the subjects of them; and the narration is not sufficiently distinct to allow the real character of the movements to be made out. And as it is proved that the severity of the operation does not produce the result, we have no resource but to believe that the cerebellum is the organ directly concerned in the combination of muscular actions required to sustain the equilibrium or to produce these movements; or, on the other hand, to adopt Mr. Noble's hypothesis, that the admitted disturbance is a result of sympathetic affection of the motor apparatus. In regard to this hypothesis, we may remark, that it is partly founded on the imperfect discrimination, to which we have already alluded, between reflex and consensual movements on the one hand, and co-ordinate voluntary movements on the other; and partly also upon the want of due discrimination between the constant *loss* of power, consequent upon the removal of the organ, and the occasional, but not necessary *disturbance* of the motor apparatus, which is equally occasioned, as we have seen, by other causes acting upon the ganglionic masses at the summit of the medulla oblongata. Between the two hypotheses, we think that the data supplied by comparative anatomy may assist us in forming a choice.

Before discussing these, however, we must make a few remarks upon the doctrine of Gall,—that the cerebellum is the seat of the sexual propensity. It seems to us that there are certain *a priori* objections to this, which have not been sufficiently considered, and which are not without weight, the uncertain character of the evidence in favour of the doctrine being a matter of notoriety. We readily admit that no *a priori* objections should stand in the face of anything like positive proof; but we cannot imagine that any candid phrenologist, who has duly considered *both* sides of the question, will affirm that such has been afforded. The first point we would urge is the evident *speciality* of the organ; not only as to its position and connexions, but also in regard to its internal structure. These seem to us to mark it out as an organ more distinct in function from the cerebrum, than the phrenological hypothesis would represent it, especially considering its very slight direct connexion with the hemispheres. For it cannot but be admitted, that the sexual propensity is as closely connected with many other propensities and desires,—e. g. with philoprogenitiveness, adhesiveness, and in the lower animals especially with combativeness,—as are any of the phrenological organs with each other; and that, in man, it has a continual operation upon the intelligence, the reasoning faculties, and the will. And yet we cannot point out any structural connexions by which this combination can be effected. Again the size of the organ, and especially its amount of vesicular matter, in comparison with the cerebrum, looking at man only, is much greater than we can fairly expect it to possess, if we contrast the degree in which this propensity exerts its influence in the well-regulated mind, with the constant and energetic operation of others. Though its force is, doubtless, greater in some individuals, and its operation on the character and conduct more constant than external observers might suppose, yet in other instances, the propensity appears to be totally absent during the whole of life. Thus it is recorded of Newton, that he never gave the slightest indication of the existence of the sexual desire. And we believe that the early repression of it by a voluntary effort, as in the case of Catholic priests, disposes to its entire cessation for the remainder of life. Now there is an utter absence of proof as to any *strongly-marked* deficiency in the size of the cerebellum,—ascertained not by external observation of the skull, but by the dimensions and weight of the organ,—in cases of this kind; in which there ought, one would think, to be an almost entire absence of it, either congenitally, or by subsequent atrophy. We are continually told by the followers of Gall, that we should form our estimate from *extreme* cases only; and yet the most extreme cases on record, as to the size of the organ in man (as ascertained, we again repeat, by direct measurement), are not to be put in comparison with the difference in relative size that presents itself as we pass from man to the animals most nearly allied to him. To one other point we have yet to allude, namely, the early development and completeness of the cerebellum in the human embryo and infant; which is very remarkable when contrasted with the evolution of the cerebrum. We refer not merely to size, but to texture; the cerebellum acquiring the adult condition in this respect at a time when the cerebrum is still very soft and pulpy. This is precisely what we should expect, if its chief office be to regulate the movements of equilibrium, &c.; but is inexplicable if we suppose the cerebellum to be merely the organ of the sexual instinct.

We shall now take a brief review of the comparative anatomy of the cerebellum, with the view of determining to which of the preceding doctrines the data supplied by it give the most support. In the first place it is to be remarked that the organ is entirely absent in the invertebrata; yet sexual instincts are manifested most strongly in their actions. Here, as in the former case, we think a distinction must be drawn between the *instinct* and the *propensity;* the former being our expression for a certain series of phenomena directed towards a given purpose, but not really involving any other physiological or psychological actions than sensations and respondent movements; the latter being a desire for the gratification, involving an *idea* of the object. If we are right in this analysis, the sexual actions of insects, &c., may be purely consensual, and may not involve a *propensity* properly so called; and the absence of a cerebellum in them is not inconsistent with the phrenological doctrine. Neither is it opposed to that of Flourens and his followers, since all the movements of these animals are evidently of the consensual and reflex character.—The class of fishes may be looked to as furnishing important evidence; because, on the one hand, we meet with a greater diversity in the development of the organ than we encounter in any other class; whilst, on the other, there are equal diversities in regard to both classes of functions referred to it. The cerebellum presents itself in some fishes as a simple lamella, implanted transversely on the medulla oblongata, behind the optic lobes. In general, however, it is more extended longitudinally, and covers in the fourth ventricle. Its highest development is attained in the cartilaginous fishes; in some of which it is marked by transverse furrows, dividing it into lamellæ more or less distinct, and thereby increasing its extent of surface. Now it is well known that in a large proportion of the class of fishes there is no sexual congress; the fertilization of the ova being accomplished simply by the diffusion of the seminal fluid through the water in the neighbourhood of the spot where the eggs are deposited—the two sexes being led by their instincts to frequent the same localities for the discharge of the contents of their genital organs. Our knowledge of the habits of fishes in this particular must be admitted to be very limited; but still there would seem a broad line of demarcation between the copulating and non-copulating fishes, which might be reasonably expected to be indicated by a difference of size or conformation in the cerebellum, if this were really the organ of the sexual feeling. Again, the diversities in the mode of locomotion of fishes are extremely great. It is to be remembered that the correspondence between the specific gravity of the water and that of the body of the animal, renders the support and the balancing of the body a matter of comparative facility in all circumstances; and the chief differences are to be looked for in the manner of propulsion, and in the facility of combining different movements for the change of direction, or for the attainment of any other special object. In the eels and other fishes of a similar shape, the vermiform character manifests itself as much in the movements as in the aspect of the body. The propulsion is almost entirely effected by the flexion of the entire trunk, the special organs being nearly undeveloped; and this flexion may well be regarded as being equally reflex in its character with the similar movements of articulated animals. But in fishes of higher orders the movements of the body become more and more complex, additional members are developed, and a much greater combination of different actions is required, in order to produce any desired result; whilst at the same time the variety of movements of which the animal is capable are very greatly augmented. This is especially the case with the most active predacious species, which are not satisfied with lying in wait for any unwary fishes that may pass near them, or with gulping down the small fry dispersed in the waters through which they move, but which pursue particular objects for vast distances, follow them with the greatest address, and adapt their movements most remarkably to the capture of their prey. There can be no hesitation, we think, in assigning to the shark tribe a pre-eminence in this respect.

Now on contrasting the size and development of the cerebellum in the copulating and non-copulating fishes respectively, there would seem, at first sight, to be something of that correspondence which might be expected on phrenological principles. For the sharks, which all copulate, possess the cerebellum in its most developed form; and the rays, which are nearly allied to them, and which also copulate, are also remarkable for the size of their cerebellum, which, though inferior to that of the sharks, is more developed than that of any other known fish. But here the correspondence ceases. The sharks and rays are by no means the only copulating fish; for a sexual congress takes place also in the chimæridæ, the syngnathidæ, the siluridæ, and in the blennies and eels. In regard to the development of the cerebellum in the greatest part of these, we have as yet no information; but it is well known that in the eels this organ is very small, being much below the average of the non-copulating fishes. Thus among the copulating fishes we find not only the highest but also a very low development of the cerebellum. Again, amongst the non-copulating fishes there are many which approach very closely in this particular to the highest group of the copulating; for whilst several of the rays are destitute of the lamellæ, which distinguish the cerebellum of the sharks, there are many of the non-copulating osseous fishes which have a cerebellum nearly (if not quite) equal in size and development to that of the rays, and far larger in proportion than that of the eels; this is the case, for instance, with the cod and the pike. These facts would seem to us decidedly unfavorable to the doctrine that the cerebellum is the organ of the sexual instinct. Let us now examine them under another point of view.

On comparing the development of the cerebellum in different species of fishes, with the activity and variety of the movements, a tolerably close degree of correspondence may be readily perceived. In the little amphioxus, which leads the life of a marine annelide, there is not a rudiment of this organ to be detected. In the lamprey and its allies, which are nearly as inert in their habits, attaching themselves to their prey by their suctorial mouths, and possessing but little power of active motion in search of it, we find the merest rudiment of a cerebellum. It is somewhat larger in the eels, which are fishes of considerable activity, though their movements are of a very simple character. In the cod and other actively swimming species, in which the impelling movements are still chiefly performed by the undulations of the body and tail, but in which the balancing and directing powers are rather given by the fins (as has been proved by experiment), we find the cerebellum attaining a much greater development; whilst in the sole, turbot, and other pleuronectidæ, which pass the greatest part of their lives with one side of their bodies in contact with the bottom of the sea, this organ is comparatively small. The highest development of the cerebellum, as already mentioned, is to be found in the sharks, a tribe pre-eminent for the vigour and variety of their movements; and in the much-dreaded white shark, it far surpasses, in comparative size and complexity of structure, that of any reptile, nearly equalling that of many birds. It may be thought that the large size of this organ in the rays (which only yield to the sharks in this particular), is opposed to our inference, since these fish are not commonly reputed to possess much energy or variety of movement; but it is to be noticed that their progression through the water is accomplished in a different manner from that of most other fishes, and really involves a combination of movement which is very unusual in that class. The rays, in swimming, make very little use of the flexure of the body, which is the chief means of propulsion in the remainder of the class; the greater part of the vertebral column being unusually solidified, and even the tail serving merely as a rudder. Progression through the water is accomplished by means of the *combined* actions of the pectoral fins; and these are exerted so neatly and easily, that the fish appears to slide along without the slightest effort. "When a skate," says a competent observer, who had no purpose to serve in making the remark, "is swimming at its ease and undisturbed, its motion is peculiarly smooth and gliding, and puts one more in mind of a kite sailing along with motionless wings, than any other motion which we are accustomed to see."* The same writer, after contrasting the varied and facile movements of the rays with the limited and laborious wrigglings of the flounder tribe, thus concludes: "We have been somewhat particular in describing the motions and organs of motion of these animals; because, on account of the peculiarity of their forms, there is a very general notion among persons not acquainted with the economy of the sea and its inhabitants, that they are very unwieldy and helpless creatures in the water; whereas the fact is quite the reverse, and, even among those fishes, which are regarded as models of perfection as swimming animals, there are none who have more command, or even as much command, of themselves as the skate." This testimony from an unprejudiced observer proves that the high development of the cerebellum in the ray tribe most completely accords with the character of their locomotive powers, instead of being opposed to it, as seemed at first sight to be the case.

The cerebellum of reptiles is, on the whole, smaller in proportion than that of the fish. In no instance does it surpass in development that of the higher cartilaginous fish; and in general it is below the average of that of the osseous fishes. Thus in the frog it is a mere transverse lamina, not covering-in the fourth ventricle, and scarcely more developed than the cerebellum of the suctorial fishes. In the lizards and serpents it is but little larger in proportion; and it is only in the crocodiles and turtles that it presents any considerable dimensions. It is well known that all reptiles have a sexual congress: though it is not always that there is an actual intromission of the male organ. Like most other acts in this remarkable class, it is very leisurely performed; often lasting several days, seldom less than one. The strength of the sexual instinct in these animals would seem to be indicated by the remarkable *preludes* which many of them perform; and by the battles of gallantry which are common among others. The frog has often been quoted by anti-phrenologists as an example of an animal possessing a strong sexual instinct, with a rudimentary cerebellum; its sexual instinct being supposed to be indicated by the duration of its copulation, and by the perseverance with which it continues to grasp the female, even after decapitation. But Mr. Noble urges (p. 330), and we think with justice, that this powerful and continuous prehension, being a purely *reflex* act, and excited (as Messrs. Todd and Bowman have shown) by the peculiar impressibility of a papillary organ developed at that period upon the thumb, cannot be taken as an indication of the peculiar strength of the sexual instinct. But he would probably admit that battles of gallantry among animals at other times peaceably disposed, or a long series of preliminary *agaceries*, such as Rusconi has described in the salamander, are unquestionable indications of the force of this instinct; and yet among most of the animals which exhibit them, the cerebellum is relatively smaller than in fishes which do not copulate at all, and in which the emission of the semen seems to be an act performed under the same conditions as the contemporaneous extrusion of the ova. The sexual passion appears to be particularly strong amongst serpents: of which, if surprised during the coitus, the male defends the female, exposing himself to every kind of peril for her safety, and submitting to mutilation rather than desert her. The iguana and some other lizards display an equal attachment; and the affection manifests itself not merely during the time of coition, but subsequently; yet its cerebellum is relatively much smaller than that of the crocodile, in which the attention of the male to the female, after copulation, seems limited to helping her on her legs again—her position during the act having been supine. There is nothing in the known manifestations of the sexual passion in this class, that can at all account for the marked superiority in the relative size of the cerebellum of the turtles and crocodiles over that of the frogs and other batrachia.

---

* Mudie, in Cyclopædia of Natural History, vol. iii. p 514.

Now, if we turn to the locomotive actions of this class, we think that a much closer correspondence is apparent. In the first place, the generally small size of the organ accords well with the very limited nature of the movements, and especially with the fact, that these movements are for the most part of a reflex character. The numberless experiments which have been made upon frogs, serpents, &c., fully prove that, not only the ordinary motions of progression, but almost all the actions performed by these animals—even such as seem the most *adaptive* in their character—are of a simply reflex nature. We should fairly expect, then, to find the cerebellum at its minimum of development, especially as, in consequence of the firmness of the support upon which the limbs and body rest in the terrestrial species, there is no need of any exertion to balance the body. The aquatic and semi-aquatic crocodiles, alligators, turtles, and mud-tortoises, however, need a much greater power of combining and regulating the movements of their limbs to sustain, balance, and propel their bodies in the fluid medium, in which, though so helpless on land, they move with great facility; and, accordingly, we find that they have the largest cerebella of the entire class. It may be objected, that there are newts and serpents which are also aquatic, and that these, nevertheless, are not distinguished by a cerebellum of larger size than that of their congeners. But to this it may be replied, that *their* propulsion is not so much effected by the combined and regulated movements of several members, as like that of ordinary fishes, by the simple undulations of the vertebral column. The inference we have drawn from the comparatively high development of the cerebellum in the turtles, is remarkably confirmed by the fact, that this superiority is confined to the aquatic species: the land tortoise, which can have little need of any co-ordinating power for its simple and stable method of progression, presenting little or no advance, as regards the development of the cerebellum, upon the average of the class.

The cerebellum makes a great advance in development in the class of birds, being not merely of much larger size, but being also marked with numerous transverse lamellæ, which indicate an extension of the surface of gray matter, and being also furnished with lateral lobes, which are the first indications of the hemispheres of the cerebellum. There is not by any means the same difference in its relative size in the several members of this group, as exists in the two preceding classes; and the inferences to be derived from its study, therefore, are less decided than those with which we seem to be furnished by the comparative investigation of its development in fishes and reptiles. Such indications as are afforded, however, by no means favour the idea that this organ is exclusively connected with the sexual passion; for we find that the polygamous fowls, with whose extensive gallantries we are all familiar, have relatively a much smaller cerebellum than the falcons and swallows, which bestow their sexual feelings on a single female. Among the monogamous birds it is very curious to see how constantly, when they have once paired, the male restricts his amorous attentions to his mate, never going astray after any other female, and how uniformly the female rejects the addresses of any other wooer, affording in this respect, as Buffon long since remarked, an excellent pattern to husbands and wives of more elevated rank in the zoological series. Yet this constancy cannot result from the relatively small size of the cerebellum, for the birds which exhibit it have the largest cerebella of the entire class, whilst the licentious chanticleer has among the smallest. Nor can it be said that birds are on the whole less amorous than mammals, in conformity with the inferior development of their cerebellum; for there are no animals in which the size and functional activity of the sexual organs undergo such a marked increase at the period of propagation as they do in birds. On the other hand, the degree of difference which exists in the relative development of the cerebellum, in the several tribes of this class, seems to accord well with the relative amount of necessity which exists for the co-ordination of different muscular actions in the various movements they execute. Thus, the birds which live chiefly upon the ground usually have smaller cerebella than those which spend much of their time on the wing; and, amongst the latter, those which pursue a living prey, and which have need of unusual power of directing and balancing their movements—such as the falcon, kite, and swallow—have a cerebellum exceeding in size the average of the class. The birds of the parrot tribe, which are distinguished by the conformation of their feet, and for the use they make of those members in prehension, also have a very large cerebellum—probably the largest in the entire class—although we are not aware that these birds are remarkable for their amative propensities.

Lastly, in the mammalia we may trace a gradual increase in the development of the cerebellum, from a grade scarcely or not at all above that which it presents in birds, to the large size and remarkable complexity which it possesses in man. This increased development is to be noticed chiefly in the lateral portions or hemispheres, which, instead of being mere appendages to the central portion, gradually come to constitute the chief bulk of the mass. The organ presents its lowest development in the rodentia and marsupialia, many of which are remarkable for their amative propensities, and for the persistence of these, under favorable circumstances, through the whole year. It is somewhat larger and more complex in structure in the ordinary hoofed quadrupeds; and its size and complexity present a further increase in the carnivora. But it is as we ascend through the quadrumanous series, from the four-*footed* rather than four-*handed* lemurs, through the baboons, the monkeys, and the semi-erect apes, up to man, that we observe the most remarkable increase. Now it cannot be affirmed, that there is any corresponding increase in the sexual propensity; for we should be sorry to believe that the well-known lasciviousness of the monkey tribes is surpassed by that of the human race, in a degree at all proportional to the relative size of the cerebellum, which is at least six times larger in the latter than in the former. But there is, on the other hand, as we ascend this series, just such a rapid increase in the number and variety of movements, which the limbs are capable of performing; with an increasing power of balancing the body upon the posterior extremities, which produces an augmented freedom of the anterior; whilst, at the same time, the development of the thumb gives a vast increase to the variety of purposes to which the hands may be applied. None but those who have studied the subject can be aware, what an extraordinary co-ordination of muscular movements is required, for the most common acts of human progression. The erect figure, the small size of the feet, the length of the stride, and a variety of other circumstances, interpose a set of mechanical difficulties, which no human skill has yet been able to overcome. We have seen automata of various kinds,—a writing and drawing automaton,—a flute-playing automaton, with divers others,—and more recently a speaking automaton; but we believe that no walking automaton was ever constructed, though the attempt has been often made. In the most remarkable collection we have seen, which included singing birds and crawling spiders,—the latter so well imitated as almost to send some of the ladies present into hysterics,—we remember to have seen one of these attempts; which, although the labour of many years had been bestowed upon it, was nothing more than a figure standing upon two legs, and advancing upon a table by the cautious *sliding* of the feet one before the other, the foot being never lifted from the ground, and not being advanced more than its own length at a time. We notice this circumstance, to show how, in this one act alone, simple as it appears, a much greater and more harmonious combination of different muscular actions is required than is involved in the progression of the lower mammalia; and we need hardly point out, we think, how remarkably man surpasses all others, in the power of adapting his muscular contractions to perform any movement which he desires to execute.

On the other hand, there is nothing either in the strength nor the constancy of the sexual propensity in man, which can at all account for the great superiority in the relative size of his cerebellum over that of every other animal. For the means we have proposed for reconciling the phrenological doctrine of the functions of the posterior lobes with the fact of their absence in the lower mammalia, and their peculiarly large size in man, will scarcely hold good here; since the formation of *ideas*, which are essential to the action of the sexual sensations upon the will, and which are associated with feelings of pleasure,—the combination of the two forming a propensity,—can scarcely be any thing else than a *cerebral* operation. And we see no reason whatever for placing the seat of the sexual sensations, and of the origination of respondent movements, any where else than in the sensory ganglia, which are evidently their instruments in invertebrata.

It is well known that, among other arguments by which Gall upheld his view of the function of the cerebellum, he maintained that the size of this organ is less in castrated animals than in those possessing their sexual organs entire. He does not, however, appear to have made any very exact observations in support of this position, which he principally rested on a comparison of crania. Subsequent observers have not confirmed the statement. Even M. Vimont admits that the result of his experiments was doubtful. Desirous to put the question to the test, M. Leuret caused his former collaborateur, M. Lassaigne, (of whose qualifications as a scientific observer, Mr. Noble expresses his ignorance,) to weigh the cerebra and cerebella of a sufficient number of stallions, mares, and geldings, and compare the results. A table of these weights, with that of the medulla oblongata, is given by M. Leuret,* for every one of ten stallions, twelve mares, and twenty-one geldings; and the result is that the cerebellum of the gelding is, on the average, *both absolutely and relatively larger* than that either of the stallion or the mare. The manner in which these results are treated by Mr. Noble is, we are constrained to say, rather too characteristic of the general mode in which phrenologists treat evidence that does not suit their purpose. In the first place, not having seen the entire tables, but only the summary of them given in Dr. Carpenter's "Human Physiology," he makes objections which a reference to the tables would prove to be unfounded. His especial ground of refusal to admit the validity of these results, is the smallness of the number of observations, and the consequent uncertainty of the averages; but if he will refer to the tables themselves, he will see that even the *extremes* are as remarkable for the evidence they afford as are the *means*. Thus, the weight of the cerebellum of the stallion is to that of the cerebrum in the stallion, as from 1 : 7·46 in the *lowest*, to 1 : 6·25 in the *highest*, the *average* being 1 : 7·07. In the mare, the proportional weight of the cerebellum is greater, not from the larger absolute size of that organ, (as Mr. Noble would make out M. Leuret to assert, contrary to what he states is the invariable fact), but from the smaller size† of the cerebrum;

---

* Anat. Comp. du Système Nerveux, tom. i. p. 427.

† We have discovered an error of calculation in M. Leuret's table, in regard to the weight of the cerebrum in Mare No 7. The number (deduced from the weight of the entire encephalon) ought to be 436, instead of 336, and the proportion 6·60, instead of 5·09.

it varies from 1 : 6·93 in the *lowest*, to 1 : 6·08 in the *highest*, the *average* being 1 : 6·59. In the gelding, the proportion varies from 1 : 6·44 in the *lowest*, (excluding one case in which the cerebrum was so enormously developed as to reduce the proportional weight of the cerebellum to 1 : 7·44, although the latter organ surpassed, in *absolute* weight, any other which is here recorded,) to 1 : 5·16 in the *highest*, the *average* being 1 : 5·97. We cannot but think that any unprejudiced person, on comparing these results, must admit that they are sufficient, at any rate, to countervail the vague statements of Gall, even though they may not be sufficiently numerous to establish an exact average of the relation. Phrenologists have too often shown a great horror of *precise* comparisons; but we submit that here is a case in which there can be no serious mistake, the cerebellum and cerebrum being so completely isolated by Nature that no accident or partiality on the part of the experimenter can affect the results in any considerable degree. We are surprised that a man of Mr. Noble's general candour and benevolence of disposition should have descended to the unworthy but easy method of invalidating the results of experiments which do not accord with his hypothesis, by imputing intentional deceit to M. Leuret, as being a known opponent of phrenology. It will be quite time, we submit, to do this, when Mr. Noble, or any other phrenologist, shall have brought before the world a more extensive series of observations made on the same plan, by which M. Leuret's results shall be overthrown. And even then it will be, of course, for the world to judge from their relative estimate of the two parties, whether the system-makers or the objectors are most likely to be in error. The results of M. Leuret's inquiries have now been before the public for some years; but as we have not heard that they have been thus invalidated, we cannot but admit them as affording important negative evidence with regard to the phrenological view of the functions of the cerebellum.

We have left ourselves no space to touch upon the bearings of pathological observation, with regard to the two views of the functions of the cerebellum. This, however, we the less regret; since the very small number of well-observed cases at present recorded, does not afford any decided evidence either way; it being quite possible, however, to put this evidence in a form that is very favorable to either side, by neglecting all that bears the other way. This Mr. Noble has done, in favour of the phrenological view; recording a small number of coincidences between injuries and diseases of the cerebellum, and disorder of the sexual organs (among others quoting a case, in which the injury was a *flesh wound* in the neck, and in which there was no proof whatever of the cerebellum being affected), but passing over with very imperfect notice the cases in which there was no such coincidence, or in which the phenomena tended the other way. He gives in detail, however, two cases recorded by Dr. Cowan, which seem to have had some influence with that gentleman,—a professed phrenologist, be it observed, but at the same time a man of too philosophical a mind to be a blind partisan of the system—in leading him to view with more favour the doctrines of Flourens. This forfeiture of allegiance to Gall, on the part of one of his followers, very amusingly excites Mr. Noble's surprise; his conviction of the truth of the system being such, that he cannot understand how any one who has once embraced it can relinquish one tittle of its dogmata. And he devotes some space to the analysis of the details of Dr. Cowan's cases, with the view of bringing them into harmony with Gall's doctrine; in this, however, he seems to us to display more ingenuity than philosophy.

On the whole, then, we are disposed to infer, from the remarkable concurrence between the results of experiment and the data supplied by comparative anatomy, that one function of the cerebellum is to regulate and combine the muscular movements required to perform a large number of our voluntary actions. And if the evidence, furnished by observation of the human species only, should prove to be such, as to establish a constant relation between the development of this organ in him, and the strength of his sexual propensity, (a position we by no means dispute, our arguments being only directed against the insufficiency of the evidence on which it rests at present) we must regard this as a *superadded* function, not as the fundamental purpose of the organ.

We conclude, as we commenced, by disclaiming any hostility whatever to the phrenological system in the abstract; and by freely admitting the *general* coincidence between the indications of human character, which are afforded by cranioscopical examination, and those derived from a direct acquaintance. But we consider that, in building up their system, the followers of Gall have been too disregardful of evidence supplied from other sources than observation of man; and that they have been misled, as to the fundamental question of the connexion of the cerebrum with the purely instinctive actions, by their inattention to comparative anatomy, which proves that the cerebrum *cannot* be the instrument of those actions; and have glossed over the important objection which the non-development of the posterior lobes in the lower mammalia and in all the oviparous vertebrata interposes to the location of the animal propensities in them. So far, however, from availing ourselves of these errors, as conclusive arguments against the whole system, we have endeavoured, by a new analysis of the propensities and emotions, to show that the facts supplied by comparative anatomy may be brought into conformity with the physiology of Gall; and that the phrenological system may be planted upon a much more secure and extended basis than it has yet possessed; a new and more exact series of observations, however, being required to build it up with anything like firmness and consistency. We cannot regard the question of the functions of the cerebellum as at all fundamental in its character; and can easily understand how a candid phrenologist like Dr. Cowan may, on this point, embrace the views held (we believe) by all the leading physiologists of the day.

Finally, we commend our review of the subject to the candid consideration of those who think with us that the determination of the *general* functions of the *encephalon* is the *first* question for the physiologist; that the determination of the share of these performed by the *cerebrum*, to be effected by attention to comparative anatomy and by experiment, is the *second;* and that the determination of the *special* functions of *different parts of the cerebrum*, to be effected (for the reasons we have stated) by the comparison of the varieties of cerebral (not cranial) conformation in man, with some assistance from that of the lower animals, is the *third*. Upon those who blindly uphold the method of Gall, in disregard of all the improvements which have taken place in our knowledge of neurology since his time, and who regard the present phrenological system as so perfect as to be incapable of improvement, we certainly despair of making any impression. We would not be supposed to assert our conviction that our own views as now expressed, are so complete as to be incapable of further improvement. Considering as we do that the whole science of encephalic phrenology is in its infancy, that a large amount of information as to the fundamental data on which it must be built up is yet wanting,—that the progress of comparative anatomy, of embryology, and of microscopy, may make many additions to our knowledge of the connexions of different parts of the nervous centres which may tend to modify previously-received doctrines,—and that, on the other hand, an entirely new system of psychological observation must be carried out, in order to bring psychology and physiology into their proper relation;—it would be absurd in us to attempt to lay down dogmatic conclusions, by which to stand or fall. We would be understood as attempting nothing in this article, but to test the relative validity of two rival *methods* of philosophizing on this subject. Our conviction of the uniformity of Nature is such, that we are thoroughly persuaded that there can be no real contradiction between her various indications, when these are properly brought together and compared; and our attempt has been to point out the application of the method, which has been elsewhere pursued with complete success, to neurological investigation. We deem it particularly incumbent upon us to point out that even if our views should be proved to be erroneous as to a few minor points,—such, for example, as the offices of the thalami and corpora striata,—our main argument is not affected. The points for which we contend are simply these:—the independent character of the sensory ganglia as the instruments of sensation and of consensual actions,—the superadded character of the cerebrum, as the organ by whose instrumentality ideas are formed and reasoning processes are carried on,—and the mixed character of the emotions and propensities, as compounded of *ideas* and the *simple feelings* of pleasure and pain. On this last point, we venture to think that we have made a real advance in psychology, which will prove to be important; and we happen to know that several intelligent psychologists are well prepared to receive it, as fixing and defining views which had been previously floating in their own minds. It seems, indeed, to have been glimpsed at by the late Mr. James Mill, in his valuable 'Analysis of the Human Mind;' his deficiency consisting in connecting the feeling too much with the sensation, rather than with the intellectual idea. We should be doing injustice to that very pains-taking anatomist, Mr. Swan, were we not to state that, on referring to his general summary of his views of the offices of the nervous centres, we find a very near coincidence with the leading features of our own doctrines regarding the relative offices of the sensory ganglia and the cerebrum;—doctrines, indeed, to which it would be easy to point out approaches in the writings of many previous physiologists, to whose authority we might refer in support of our own.

Art. XIV.

On the Injurious Effects of Mineral Poisons in the Practice of Medicine, &c. &c. By Horatio Prater, M.D. Ph.D. &c. 12mo, *London*, 1846.

If for once in our lives our gravity might be pardoned for perpetrating a manifest and facile pun, we would say that the work before us is clearly the production of a *Prater*,—an arrant prater of other men's nonsense and his own. It professes to contain an epitome of and commentary on the dietetics of Cornaro, and on the new system of medicine of Raspail, "embracing all the details necessary," as Dr. Prater tells us, "for persons to give this system a fair trial," &c. &c. It is a shrewd compound of twaddle and stuff, intended to be very attractive to noodles and old women, whether in petticoats or breeches. There are some good jokes in the thing, however,—not jokes by the author, who is a dull fellow enough, but statements which his amazing stolidity and ignorance render ridiculous; statements which would figure well in a farce if delivered seriously and in character.

Our author's system of cure participates in the merits of all popular systems, without any of their imperfections. Morison and Mesmer, Priessnitz and Hahnemann, Dickson and Raspail, are all plundered of their finest plumes. Dr. Prater, for example, would give, according to his system, those substances only as medicines which are found as component parts of the body. He would use them separately, but in the very small proportions in which they exist in the human body; and this Dr. Prater observes, makes his system very similar to the homœopathic. He would also increase or encourage the first symptoms of diseases by medicines which induce similar symptoms, especially when the alimentary canal is to be thoroughly cleared out. "But after this is done, we would not continue (as we presume the regular homœopathist would) the use of medicines tending to keep up such actions, but rather the contrary, since, as we have already stated, such symptoms may often keep up, from the effect of a bad habit (so to speak) the vital principle has got into." (p. 14.)

Dr. Prater has an ingenious proposition respecting keeping up "connexions by blood." He thinks the old folk should have transfusion practised from the veins of their offspring. "If there be any thing," he says, "approaching the character of an *elixir vitæ* in nature, it seems more reasonable to look for it in the blood of the adult and healthy offspring than perhaps to any other source." He would also give "oxygen water and solution of deutoxyd of hydrogen." But further—"Eggs and oysters, as evidently *containing a vital principle*, (the italics are not ours,) may be used in such cases, for from their exciting nature they seem, perhaps, to impart a certain quantity of this to those who use them as food." Nay, he holds more recondite views than these, and thinks other means worthy of trial. "Whether, on similar principles, there can be a communication of nervous influence can only be learned by experiment. Does the optic or frontal nerve of an animal *just killed*, laid on the upper eyelid, benefit amaurosis? or the spinal marrow of the same laid or rubbed on the spine of the debilitated old man give any additional strength?" (p. 19.) Aye, answer this, ye who can! We certainly cannot, though we doubt not our great philosopher could if he chose.

Raspail is a sort of monomaniac who has given himself up to the influence of the idea that nine-tenths of all diseases are dependent on parasitical animals or worms; an idea, we believe, not unusual amongst medical monomaniacs. The ova of these creatures can penetrate every part of the body; for example, those of the ascarides may enter the lungs with the air at each inspiration. Ascarides never lay their eggs in fæces, "except" (poor things!) "in cases of despair." Acari constitute another very insinuating class of parasites. They will get into flour and cheese, revel in the cracks of wooden pitchers, &c.

> "They live in the joinings of *old* furniture, and in houses old or dirty, old wax, collections of dried plants, or animals, in ill-conditioned ulcers, and wherever the *fermentation caséique* can develope itself. Hence it would appear right that the joinings of *all old bedsteads* should be well examined not only on account of bugs, but also of these acari; and no doubt it is chiefly to prevent the entrance of these into the mouth or nostrils that Raspail uses his cigarette of camphor *during the night*, when, as we have observed, they generally begin to move about." (p. 41.)

This theory will inevitably recall to the mind of every one who read medical books so long ago as the year 1810, the history of the man in Haslam's most amusing *Illustrations of Madness*, who always slept with his head in a tin saucepan, "to prevent the intrusion of the sprites."

Enraptured by the vermicular theory, our author has given vent to his feelings in immortal verse, and has printed a portion of his poesy. We subjoin a sample for the delectation of our readers:

"We have heard of embalming after man's death,
When for ever has left him the vital breath.
Thus the dead man is kept, no doubt, from decay,
And all sorts of vermin chased from him away:
But here, in this book of Raspail, you will read
That *during our life* of embalming we've need;
That insects assail us within and without;
That from these come all ills—both fevers and gout;
That CAMPHOR and ALOES these only can save us,
And keep that life sound which great Nature gave us.

. . . .

We know some love smoking—smoke half the day;
To all our great smokers here then we say—
Adopt this new plan, smoke, too, *all the night*
Your camphor cigar, which ne'er wanteth a light!
Then by you, *in your life*, no worms will be fed—
Quite enough to be eaten by them *when you're dead!*"—(p. 49.)

Dr. Prater thinks that a deficiency in the quantity of saline matter in the system may be a principle cause of a great many diseases; "in fact, of all those that have an animate origin." He would rather, for his own part, "take a little less aloes and camphor, and a little more saline medicine as *preventives* of worms, than what Raspail recommends." Raspail is good; Prater is better. Nevertheless, Prater likes the notion of ladies smoking camphor cigarrettes in bed; but if this be impracticable, "a piece of camphor inclosed in a linen bag, and placed under the pillow, or tied so as to hang near the mouth and nostrils, would probably be amply sufficient to keep acari and all night vermin from getting into the nose, mouth, or ears, and depositing their ova there." There is one property of camphor, mentioned by Dr. Prater, which might have induced him to caution certain of his readers to take care and make the bag fast, so that it should not slip away and get down into the bed: that property is, "its remarkable power, when sprinkled in powder over the nates, of taking away all venereal appetite, as admitted by many old authors, and by Raspail himself"!

We could fill pages with drolleries like these; things so solemnly writ, and yet so absurd, that the reader would half suspect the whole matter to be a piece of irony, were it not that the asinine stolidity with which it is set forth bears internal evidence of genuineness. And yet there is every now and then bubbling up from his dark depths a bit of brilliant philosophy so very naked as would pass for cunning in other men, but which must convince, as it is intended, every reader that our great Horatio, at least, is a truly modest, simple-minded, truth-loving philosopher of the first water.

For example, he finds in the squabbles of the Royal Society a good reason for glorifying himself. Referring to the clamour lately raised respecting the adjudication of the medals, he observes, "the calm consideration of these unjust proceedings, and a firm conviction of the gross injustice of many writers, and the ignorance of others, have induced me to enumerate my humble labours on the title-page of the present work." And, sure enough, on the title-page, we have appended to the name of our author the following modest enumeration of what he was—even before the publication of the present treatise, videlicet:

> "Horatio Prater, M.D., PH.D., Discoverer of the Fusible Compound of Carbon and Silica; of the peculiar Agency of Lime on the Tonicity of Muscular Fibre; of the Permanent Fluidity of the Blood by a Heat of 140°; of the Acceleration of its Coagulation by Ammonia, Carbonate of Soda, &c.; of the *Cause* of the Coagulation of Albumen by Heat; of the Diffusive Power of the Gases; and, conjointly with Fizeau, of Möser's Images; and Author of 'Experimental Inquiries in Chemical Physiology,' &c. &c. &c."

Alas, alas,

"The world knows nothing of its greatest men."

Who, before reading these affecting details, could have imagined that such a genius was lying *perdu* amongst us? And yet who, after reading the great work now before us, can doubt that the ill-used author is much more than is here set forth.

"Horatio, thou art e'en as queer a man
As e'er our observation coped withal."

ART. XV.

A Practical Treatise on the Diseases of Children. By JAMES MILMAN COLEY, M.D., Member of the Royal College of Physicians in London, &c.—*London*, 1846. 8vo, pp. 467.

WE regret much that we cannot speak of this work at all in terms of commendation; and we confess we cannot see why it was written, or, at least, why it has been published. The preface, it will be seen, is one of considerable pretensions, both with reference to the author and his book; but so far are we from thinking the book deserves to be regarded in the light in which Dr. Coley wishes to represent it—"as a comprehensive work of reference"—that we cannot give it even the credit due to a well-executed compilation.

The surgical diseases of childhood present, with the few exceptions of original malformations, no, or scarcely any, peculiarities requiring special mention, though, in his preface, the author takes much credit to himself for having noticed surgical diseases. And we regret to say that what he professes to do he has done ill. For example, he describes the operation for strangulated hernia, while all the very important questions as to the time when a truss should be applied, how you can best avoid the annoyance to a young infant inseparable from a truss becoming soiled with its discharges, the best form of truss, &c.—questions of real importance, since the age of the patient here really does modify our practice—are left unnoticed. Nothing is said about non-descent of the testicle, &c. &c.

At page 69, external hydrocephalus is used as a synonym for cephalhæmatoma! while this accident is noticed in the most cursory manner. The author speaks (p. 8) of *many* instances of success of the operation for imperfect anus, when such instances are notoriously very few, &c. &c. If from the chapters on diseases of the eye you take away all that is Mr. Lawrence's, and all that is Mr. Middlemore's, very little will remain for Dr. Coley. From the seventy pages on diseases of the skin we have learned nothing. In the preface the author glorifies himself for his notice of erysipelas. It is only during infancy that we find deviations from the ordinary course of erysipelas, such as—that it has a greater tendency to wander—that it often proves fatal in young children without either vesication or the formation of matter—that the former result is much rarer than the latter, &c. Dr. Coley notices none of these; and we find that the treatment recommended is what might be adopted for a man of 20 or 30. What guide would his directions be to a pupil who had to treat an infant of a month old? The important subjects of measles and scarlatina are slurred over in an extraordinary way; and his remarks on the dropsy after scarlatina are absurdly short.

But it is wearisome to go on noticing a book in which we can really find nothing to praise. Take, for instance, the chapter on gangrene of the mouth (p. 131). A page and a half are occupied with the translation of three post-mortem examinations by Rilliet and Barthez; the remaining two pages and a half are inferior to what might be expected from a second year's student. Immediately after these follows a notice of "Cancrum oris, sloughing phagedæna of the mouth;" in which the author appears to have confounded simple stomatitis with the affection he has described in the preceding section.

The article on dentition consists of four pages of common-place physiology. Then comes a scrap of French, which, as in many other instances, Dr. Coley leaves untranslated. Many little bits of French are thus introduced, mere scraps, often of no importance whatever (see p. 253). He denies that any other diseases than local inflammation are connected with dentition, and yet he does not notice at all that general inflammation of the gums of teething children, which any patient observer must often have noticed, and which has been described as Odontitis. No help is given towards fixing any rule about lancing the gums, though this is surely a matter on which, after forty years' practice, a man must have arrived at some conclusion.

There is no opinion given as to the advantage of the early operation for harelip (p. 152), though the subject has of late received much attention in France.

In his account of the diseases of the digestive organs, Billard, Rilliet, and Barthez have been drawn on very largely, but there is a popular physiology of his own very often introduced, which we do not at all like. See, for instance, the account of the anatomy and physiology of the digestive organs at pp. 165, 175. Whom is this meant to teach? It is certainly more adapted to the capacity of the monthly nurse than the medical practitioner, or even the most juvenile student.

The chapter on croup is, on the whole, a favorable specimen of the book; but what is there in it? It teaches nothing new, nor gives any weight to what was known before. In the chapter on pneumonia (p. 281) we have a description of all sorts of absurd and even impossible symptoms; the expectoration described in children who cannot spit, and delirium talked of in infants who have not yet attained the use of reason. The morbid appearances are entirely from Rilliet and Barthez, as is the case also in the article on phthisis. The important and extensive subject of diseases of the brain is compressed into the narrowest limits, and is as unsatisfactory as possible.

The general style of the work is slovenly, sometimes ungrammatical. Misprints abound, while there is a great attempt at a display of learning both in the text and notes. If, however, the reader will look at the wondrous authorities (German) quoted in the notes of pages 151, 250, 253, 264, &c., he will assuredly feel much doubt as to whether the author has ever seen the books he quotes, or could read them if he had.

Before concluding, we must say one word on the title-page of this book. We should be glad to be informed on what authority the author appends the M.D. to his name. We happen to know that he is not a graduate in medicine in any university or other institution qualified to confer the degree and title of "MEDICINÆ DOCTOR;" and we have always been taught to believe that a person who has not so graduated, is no more entitled to place after his name the two letters M.D., than he is to place the three letters KNT., or the four letters BART. The author of this volume is a LICENTIATE, or, as he writes it, a MEMBER of the London College of Physicians; and he is unquestionably legally qualified to practise as a physician; but the College neither has, nor pretends to have the power or authority, to grant to any one the degree of *Doctor in Medicine*, and therefore we regard the assumption of this title and dignity by a mere licentiate as altogether unwarrantable. As the licence of the College qualifies the holder, as we have said, to *practise* as a physician, it would not perhaps be overstraining the privilege, if such holder took *the name* as well as the office of physician. At least, we should ourselves not only readily accord this title, but we would even, out of courtesy, give the name of *Doctor* also, just as we give this name to Bachelors of Medicine. But just as no one ever saw a Bachelor of Medicine, however long he may have been *named* DR. in common parlance, place the talismanic letters M.D. after his name, so, *à fortiori*, no one ought ever to see an ungraduated licentiate of the College of Physicians to do so. Dr. Coley—as we are willing to style him by courtesy, though we believe his proper title to be Mr. Coley, Physician,—can certainly plead, in extenuation of his fault, that he is kept in countenance by many other of his fellow-licentiates, both *intra et extra urbem;* but it is principally on this very account that we notice the subject here, and we say—*peccatur intra muros et extra.* We know Dr. Coley to be a gentleman of the highest respectability; and though he has written a bad book, we believe him to be an experienced and skilful physician. No Doctor of Medicine, at whatsoever university he may have graduated, could hesitate for a moment to meet him in consultation; none, we should hope, would refuse to address him by the ordinary title; but it is well that the members of the profession generally, and the Fellows of the College of Physicians especially, should be made aware that there are to be found among the ungraduated licentiates of the college, men of a very different stamp, who are a disgrace to the title of *Doctor of Medicine*, which they so improperly assume—men who are not only utterly undisciplined in all that literary culture which a *degree* and its concomitant *title* are usually understood to imply, but who are even destitute of that amount of special knowledge which ought to be possessed by every member of the medical profession, however humble. Can such things be—and overcome us like a summer cloud, without our special wonder? But if any of our wondering readers should be curious also, and desire to know *how* such things be, we refer them for particulars to that famous statute which established the ELECTS of the College of Physicians.

PART SECOND.

Bibliographical Notices.

Art. I.—*Notes and Recollections of a Professional Life.* By the late William Fergusson, Esq., m. d., Inspector-General of Military Hospitals. Edited by his Son, James Fergusson.—*London*, 1846. 8vo, pp. 248.

After having served with distinction for twenty-three years, during the stirring and eventful period between 1794 and 1817, and having attained the rank of Inspector-General of Army Hospitals, Dr. Fergusson retired into civil life and established himself, first at Edinburgh, and, four years afterwards, at Windsor, where he enjoyed an extensive and lucrative practice, till attacked with paralysis two years previous to his death. He possessed ample opportunities during his military career of studying the diseases of soldiers, both on the Continent and in the West Indies, in the field and in garrison, in the time of peace and during war.

> "I embarked," he remarks, "nine times from the shores of Britain with armaments on foreign expeditions, and, during twenty-four years' actual service, I spent seventeen years, or parts of them, in other climates, passing through every grade of medical rank in every variety of service, even to that of the sister service of the navy; and it thus was my fortune to have sailed in every ship of war, from the first rate of the line down to the smallest craft that carries a pennant."

Dr. Fergusson contributed many able papers to the medical journals, on subjects connected with the public health, and more particularly on those relating to military hygiene. The latter half of the volume before us consists of a reprint of some of the more important of these contributions, while the first part contains a series of papers, commenced shortly before his illness, and which was intended to form "a digested abstract of opinions and observations that have been subjected to the ordeal of long and deliberate consideration." Unfortunately he was not spared to carry out his views on this point. Of the papers he had completed, some refer to the state of the army as it was when our author served in it, and exposes abuses which happily no longer exist, while others are perhaps too purely military to demand lengthened notice from us. We would recommend the perusal of the work to every army medical officer who feels really interested in the soldier, and takes a pride in the service to which he belongs, as the production of an able and intelligent officer, and containing many excellent observations on subjects connected with the health of troops; it has also the advantage of being written in an amusing and very readable style. We shall content ourselves with one quotation from a paper "On Fever as an Army Disease," which we believe contains the true secret of its successful treatment, and may suggest useful reflections to those who are instant in season and out of season with their endeavours to cut short and to cure it by heroic remedies.

> "It is the *gastro-enterite*, cry the disciples of Broussais, and there can be no cure but the leech. It is cerebral inflammation, respond the followers of Clutterbuck, and the only remedy is venesection. Purgatives are the true treatment, proclaims the Edinburgh school. They are irritating and dangerous, replies that of London. In fact, all are equally right and all are equally wrong if they fail to note times and seasons, the nature of the epidemic, and the characteristic tendencies of the patient's constitution, his powers to bear the operation of medicine, and his ability to resist the tendency to death. There can be no treatment of fever by physic but in studying the *juvantia* and *lædentia* of the case—cultivating the first, eschewing the last, and never forgetting that there is a mighty power always operating in your favour—the *vis medicatrix naturæ.* Do not thwart her beyond the mark, and she will get you through difficulties with which, without her aid, you could not cope; but the physician who believes that he possesses beyond these, medicines of specific power in fever, really should have his own licence suspended, and himself put under cure until the monomania subsides. . . . . . . The battle is to be fought by the nurse, whether in the shape of physician or other attendant it matters not. Only let that attendant be sagacious and diligent, and the patient is saved—the contrary, and he dies."

ART. II.—*The Microscopic Anatomy of the Human Body in Health and Disease, illustrated with numerous Drawings in Colour.* By ARTHUR HILL HASSALL, F. L. S., M. R. C. S., &c.—Parts I and II. 8vo, pp. 76. With six Plates.

THE design of this work is very commendable. The present rapid advance of microscopic inquiry, together with the increasing importance of its results, fairly demand that a special treatise should be devoted to it; and a volume so moderate in size and price as this promises to be, would deserve every encouragement if its performance were equally satisfactory. This, however, we regret to say, is not in our opinion the case, in regard to any one of the points on which the author claims a favorable consideration. After stating the intended number of parts to be twelve, he proceeds—"It is intended to embrace a systematic and copious *illustrated* description of the various fluids and solids of the human frame, no structure or organ of the body being omitted." And he afterwards adds, "The design of the work, if not altogether original, is almost unique; the only work extant which embraces the entire range of microscopic anatomy, and this *not exclusively human*, is that of Mandl." Why, Mr. Hassall's work is not a whit more exclusively human than that of Mandl. Of the *three* plates in the first number, only *one*—and of the *three* in the second number, only *half a one*—represents objects of human anatomy. We should not think the worse of the book on this account, if it were not that if the other subjects are treated upon the same scale with the blood and circulation (which occupy the two parts already published), the book must extend to fifty parts instead of twelve; and that the fact is plainly inconsistent with the author's preliminary announcement. Further, Mr. Hassall says, "One great feature of the book, it is hoped, will be the *fidelity of the drawings;* a principal fault in most of the published works on minute and microscopic anatomy being that the figures do not exhibit the characters of the text." Here, again, we feel obliged to state that Mr. Hassall's figures are *very* far from bearing a resemblance to nature, either in their drawing or colouring. The drawing is in coarse lithography, which is not at all calculated to give an idea of the real aspects of the objects. And the colouring will strike every practised microscopist, we venture to affirm, as egregiously incorrect, and calculated to mislead. In all the figures of red corpuscles of the blood, for example, these objects are represented as of a full red tint, even when lying in a single layer; when every tyro knows that the hue is scarcely distinguishable in a single disc, and that the red hue becomes apparent only when two or three overlap. The differences between the red and colourless corpuscles of the human blood, in the very first figure, are thus grossly exaggerated; as is manifest from the fact, that no one could overlook them in the plate, whilst in nature they have only been lately detected by a careful scrutiny. We recommend to Mr. Hassall's attention the admirable figures of Donné, which are engravings of high finish and great beauty, faithfully copied from daguerreotype images of the objects themselves; as showing what blood-corpuscles really resemble under the microscope.

We greatly regret that we are obliged to speak thus disparagingly of Mr. Hassall's very laudable attempt. But our duty to the medical public requires, that as the subject is one on which, from the attention we have given to it from the very first, we feel well qualified to form an opinion, this opinion should be expressed as unreservedly as if it were more favorable. Should future numbers present such an improvement in their character, as to warrant our commendations instead of our condemnation, we shall most gladly bestow them.

ART. III.—*Elements of Physics.* By C. F. PESCHEL, Principal of the Royal Military College at Dresden, &c. Translated from the German, with Notes, by E. WEST. Illustrated with 430 Wood-engravings.—*London*, 1845–6. Three Volumes, 12mo, pp. 894.

"THE study of the physical sciences," says the translator, "has become of late almost universal; partly because it has been found well suited to cultivate habits of observation and reasoning, which cannot fail to be of use in the business of life, and partly because the practical application of scientific principles to the details of almost every department of manufacturing and agricultural industry has rendered some acquaintance with natural philosophy and chemistry indispensable." We need not, we trust, now dwell upon the importance of these studies to the youth who is preparing himself for the medical profession; since our opinion of their value has been frequently and strongly expressed. Continued observation, indeed, has only served to convince us yet more decidedly that the *direct* benefits of these pursuits to the medical student, great as these are, are the least of their advantages; and that the philosophic training which they are calculated to afford is, if due pains be taken by the teacher to apply it, the most important result of attention to them. But this result cannot be attained by the acquirement of a mere smattering of acquaintance with physical science, such as may be very useful as regards its practical applications; it requires for its development a closer and more patient study, such as few youths in this country, save those who go through a regular university course, devote to the subject. We quite agree with the translator that, notwithstanding the multiplicity of elementary treatises on physics, there was yet room for another, which, without aiming at being a *popular* introduction, should be sufficiently simple for an intelligent beginner; but which should employ the acquaintance with elementary mathematics, which most schoolboys now gain, as a means of introducing a more scientific method of treatment, such as is at present confined to works of the highest class. And though we cannot doubt, from the excellent manner in which he has translated and edited the work of Peschel, and from the character of the additions which he has made to it, that he was fully competent to produce such a treatise for himself, yet he has not done unwisely, perhaps, in availing himself of one which so admirably fills up the deficiency alluded to.

The first volume, published rather more than a year ago, and containing the physics of ponderable bodies, has been most favorably received by those best qualified to form a judgment of it; and has, we believe, been introduced as a text-book into several of our leading academies. It would, therefore, be superfluous in us to offer any opinion of our own upon its merits; but we may state that a careful examination of the other two volumes, which have recently appeared, has satisfied us that their merit is equal, if not superior, to that of the first; and that the whole book may be most safely recommended to those who desire to attain something more than an elementary knowledge of physical science. It will not supersede the excellent Manual of Dr. Golding Bird, which is more concise in its character and limited in its range, and which is better adapted, therefore, for those who can spare but a small modicum of time for such pursuits, or who are, unfortunately, not sufficiently well grounded in mathematics to avail themselves of the higher advantages of the treatise before us. But we trust the day is not far distant, when every medical student, who aims at taking a respectable stand in his profession, shall have made himself fully master of Peschel's 'Elements of Physics,' even if he proceed no further.

Art. IV.—*The Vegetable Kingdom; or the Structure, Classification, and Uses of Plants, illustrated upon the Natural System.* By John Lindley, ph.d., f.r.s., f.l.s., Professor of Botany in the University of London, and in the Royal Institution of Great Britain. With upwards of Five Hundred Illustrations.—*London*, 1846. 8vo, pp. 908.

This work, which we may really call a splendid one, may be considered as a third edition of Dr. Lindley's Introduction to the Natural System of Botany, of which the first was published in 1830. At that period, the system of De Candolle was in general estimation amongst those botanists who had the wisdom to discern the absurdity of adhering to the Linnæan or artificial system, long after its author would have disowned it. The progress of science, however, showed many imperfections and inconsistencies in De Candolle's arrangement, particularly as regarded the want of some intermediate subdivisions between the few primary groups of Phanerogamia and the hundreds of natural orders which were even then recognized. And in his second edition, published in 1836, Dr. Lindley attempted to carry out a plan, which had been proposed by some continental writers, of grouping these orders into alliances. The ten years which have elapsed since that period, have served to confirm the general truth of the views which he then propounded, whilst they have rendered necessary many modifications as to details. The new materials continually presenting themselves to the botanical architect, for which a place *must* be found in his edifice, require continual shiftings and alterations in his minor arrangements, although the principles of construction may remain the same; and consequently it is as absurd to criticise him for departures from his former plans, as it would be to find fault with a chemist or a physiologist for making discoveries which upset or modify some previous system. "In fact," as Dr. Lindley justly remarks, "there is no such thing as stability in these matters. Consistency is but another name for obstinacy. All things are undergoing incessant change. Every science is in a state of progression, and of all others the sciences of observation the most so." And with most commendable humility he proceeds: "The author's object, and he thinks he may say that of every one else who has turned his attention to the question of late, has not been to establish a system of his own, which shall be immutable, but to contribute to the extent of his ability towards that end. . . . All that we can do is to throw our pebbles upon the heap, which shall hereafter, when they have sufficiently accumulated, become the landmark of systematic botany."

The following outline of the contents of this volume will indicate the vast amount of matter contained in it, and will give some idea of the labour which must have been bestowed upon its composition:

> "Its object is to give a concise view of the state of systematic botany at the present day, to show the real or supposed relation of one group of plants to another, to explain their geographical distribution, and to point out the various uses to which the species are applied in different countries. The names of all known genera, with their synonyms, are given under each natural order, the numbers of genera and species are in every case computed from what seems the best authority, and complete indices of the multitude of names embodied in the work are added, so as to enable a botanist to know immediately under what natural order a given genus is stationed, or what are the uses to which any species has been applied. Finally, the work is copiously illustrated by wood and glyphographic cuts; and, for the convenience of students, an artificial analysis of the system is placed at the end." (Preface, p. xii.)

It would be quite beyond our province to attempt any detailed criticism on such a masterly compendium of botanical science as the one now before us. And we shall content ourselves with recording our conviction, that no work of any thing like equal value exists either in our own language or in any other; that it is consequently indispensable to every student who desires to make himself acquainted with the present aspect of botanical science, besides serving as a most valuable work of reference to those who desire only occasional information as to the characters, properties, and uses of the various species of plants (to the acquirement of which the copious indices give every facility); and that Dr. Lindley well deserves the gratitude of all botanists for the zeal and perseverance which could alone have carried him through a work of such immense labour.

We cannot put aside the book, however, without remarking that, in our opinion, Dr. Lindley's attempt to Anglicize scientific names may be very well spared, and is in fact injurious. In a *popular* treatise, it is very right to endeavour to ingraft science upon facts of familiar knowledge, and to show that the many-syllabled Euphorbiaceæ and Ranunculaceæ are nothing but Spurges and Crowfoots, or plants allied to them. But in a scientific work, intended for foreigners as well as natives, the common language of science ought, we think, to be rigorously maintained; and we cannot see the advantage, either in euphony or appearance, of Hydrocharads over Hydrocharidaceæ, Bromelworts over Bromeliaceæ, Pontederads over Pontederaceæ, or Kadsurads over Schizandraceæ.

Art. V.—*The Anatomy of the Arteries of the Human Body, and its Applications to Pathology and Operative Surgery; with a series of Lithographic Drawings the Size of Nature.* By Richard Quain, F.R.S., The Drawings by Joseph Maclise, Esq., Surgeon. Eighty-seven Plates, imperial folio. The Letter-press 8vo, pp. 550. *London*, 1844.

Although we have already given a full and elaborate analysis of this incomparable work (No. XXXVIII, April, 1845), we are induced to notice it once more, from the circumstance of its now coming before us, virtually, though not actually, as a New Edition, and because we feel it a duty to our readers, to make them acquainted with the splendid prize now within their reach. The publishers, in their new Prospectus, inform us "that as the large size of the stones on which the drawings are made, renders it difficult to retain them uninjured for use from time to time, whenever they may be wanted, they propose to throw off at once 500 copies, and then efface the drawings from the stones, that the plates may not be produced in a less perfect manner." When subscribers are received for these copies (500), the work will be delivered at the price of Six Guineas, in place of the original price of £10 12s. This, we will venture to say, from our own knowledge, is one of the cheapest publications ever offered to the profession; and we earnestly advise *every surgeon who operates or expects to operate*, not to lose the present opportunity of possessing a work which may be really said to be almost indispensable. The *possibility* of obtaining it can only exist for a short time, as there cannot be a doubt but the small subscription list of five hundred will soon be filled. We would suggest to the publishers to offer the work even at a less price, say *five guineas*, provided the subscribers (as seems likely) greatly exceed the stipulated number of five hundred.

PART THIRD.

CONTRIBUTIONS TOWARDS THE ADVANCEMENT
OF THE
Natural History and Treatment of Diseases.

I. ON EXCESSIVE TRUST IN NATURE IN THE CURE OF DISEASES.

BY J. A. SYMONDS, M.D.,

Consulting Physician to the General Hospital, Bristol.

(*In a Letter to Dr. Forbes.*)

Bristol, August 20, 1846.

My dear Sir,—I was much gratified by your remarks in the Journal for April, obviously intended to counteract some of the impressions which had been produced by your article in the January Number. I knew you to be a vigorous eclectic in practice, but I feared that the vehemence of your protest against the manner in which the *medicina perturbatrix* is sometimes exercised, and that the pains which you took to hint or assert the adequacy of Nature alone to the cure of many and very important diseases, might be prejudicial in two ways. In the first place, I apprehended that your frank and plenary confessions as to the inutility or injuriousness of some of the ordinary methods of cure might be caught up, not only by ignorant and pestilent empirics, but also by the fanatical apostles and disciples of the homœopathic and hydropathic heresies; and that the confidence of the laity in legitimate medicine might be thereby shaken more than it has been already. This *læsa fides* is no small addition to the difficulties and perplexities which, under the most favorable circumstances, must often beset the honest and scientific practitioner. On the other hand, I was fearful that your recommendation of a trial of the undisturbed play of the *vis medicatrix* might induce in some minds a general indolence of practice, and in others might encourage that liability to paralysis of judgment, which, by the bedside, has often produced absolutely fatal results, both to the patient and the practitioner. I know that you maintain that there are many cases in which, with our present knowledge, it is our bounden duty to exercise great vigour in our interference with disease. We must act up to our lights. Be they strong or be they feeble, we must still act up to them. The *laissez-faire* system is good, in so far as it gives free scope to healthful energies; but, if it presumes to tolerate the powers of darkness, the sooner it goes to Tophet, the better for poor man, who has hard work enough as it is in fencing off disease and the devil.

I repeat, then, that it gave me great satisfaction to find that you had so distinctly expressed your belief in the necessity of an active interference with disease, and in the efficacy of many of the received remedies, and that you wished to be understood as having only entered a strong caveat against the blind heroics and unreasonable polypharmacy still too prevalent, and as having suggested a more thorough and searching inquiry into the natural history of morbid phenomena, and into the principles upon which therapeutics should be founded.

Your essay elicited some very interesting correspondence—Dr. Laycock's paper must have been read with the same satisfaction as the many other contributions which have been bestowed by that enlightened physician on the philosophy of medicine. The letter to which the honoured name of Dr. Andrew Combe is attached, and the anonymous epistles in your last Number, appear to me to set rather too much in the direction of the expectant method. You will therefore, perhaps, excuse me, if, at the risk of uttering things trite and tedious, I venture to make a few remarks, the drift of which will be opposed to the non coercive system of treating diseases.

The question before us is, whether it is safer to leave diseases to their own course, or to interpose what we call remedies? And really this is a very frightful question to be asking at the present date of the world's history, when medicine has been proposed as an art two thousand years and more! What! is it a probability admitting of debate. that, of the countless swarms of ghosts, that have passed "the melancholy flood," medicine has introduced to the "grim ferryman" those whom kind Nature would have held back? But we need not frighten the imagination with horrible surmises of forthcoming corollaries from a possible conclusion, which, if it were true, would be one of the mournfullest truths in the universe.

I confess that Dr. Fleischmann's table somewhat startled me—for a moment it seemed to be the most awful statement in numbers that I had ever met with. I say awful, because, if received as it stands, it would require us to revolutionize from the foundations the whole constitution of medical art. To think of 100 recoveries out of 105 cases of peritonitis!—with no treatment but what was dietetical, for I entirely agree with you in computing the globules as = 0. What sort of disease peritonitis may be in Austria, I have no knowledge from experience; but, if this statement be correct, I have no hesitation in expressing my belief, that it must be a different malady from that which is associated in my mind with the name. But, on further examination, I noticed anomalies which soon removed the discomfort which the first view of the table occasioned. There appear to have been only six cases of enteritis. This may perhaps be accounted for by a restriction of the name of inflammation of the muscular coat of the intestine. There is no mention of enteritis as seated in the mucous membrane, and I therefore infer that it is included under diarrhea. Yet, strange to say, this last-named category numbers only 114 cases—that is, only 9 more than peritonitis. Again, while 300 are assigned to pneumonia, bronchitis has only 15! At first it seemed probable that the milder cases of disease are not admitted into the hospital; but, were this the case, how are we to explain 300 affixed to tonsillitis? *Gout* appears to be common among the paupers of Vienna;—possibly the paternal government makes them luxurious. There are no less than 102 cases—more than half the number ranged under the head of rheumatism (183). These *peculiarities* of Dr. Fleischmann's table are, I think, sufficient to justify us in excluding it from the present inquiry.

Whether a true art of medicine does or does not exist, it must be allowed that there *ought* to be one. Man has succeeded in conquering or repressing so many of the adverse powers in Nature, and has subjugated so many others to his purposes, that we might by analogy expect him to acquire some dominion over the *vis vitiatrix* and the *vis necatrix.* The existence of such forces in Nature is apt to be forgotten by physicians, who, from the influence of the old Hippocratic doctrines, got into the habit of representing Nature, φυσις, as all-beneficent—as if she had no poisons, generated no diseases, and allowed nobody to die if she could help it. Now, it would be contrary to all other experience under the sun, if it should be discovered that men of genius and patient observation have for two thousand years been watching a certain set of phenomena with a view to altering them and making them serve their purposes, and have tried a number of experiments to that effect, and yet have accomplished nothing, or next to nothing! Let it not be said that, though men have been so successful with the mechanical and the fine arts, like results were not to be expected when they had to deal with *vital* phenomena. Has nothing been done in agriculture, horticulture, and the breeding of animals, &c. &c.? Art, after all, is but Nature in a new form—a fresh arrangement of the forces of Nature, compelling them to work under new conditions.

Much of the practice in the ordinary system of medicine is a strict imitation of salutary processes in Nature, and its object is to make such processes supersede the *baneful practice of Nature*, who, though she knits up wounds with her adhesive inflammation, by the very same method glues the intestines into fatal entanglements, shackles the heart, and chokes up the windpipe. In *this* man she soothes "the grief of a wound" by pouring out serum; in *that* she makes the same effusion effectually close the rima glottidis. She spirts blood from the hemorrhoidal vessels of Paul, who blesses her for saving him from apoplexy; of which very disease poor Peter dies, because she has poured the same fluid into his lateral ventricles. And so on, for man's body is a microcosm in which one sees the play of Zoroaster's antagonist principles. Nature is ever the same. Blessings are mixed with curses—the poisonous berry and the nutritious root are found in the same plant. There are balmy dews and pestilent fogs—fertilizing streams and destroying deluges—and the lair of the murderous lightning is in the cloud that floats across the blessed sunshine. But man is sent into the world to wage eternal war with the evil one—and woe betide him, if he lets the demons of disease get an advantage over the angels of reparation, for want of his friendly and judicious interference.

I think, then, that there *ought*, in conformity with man's other achievements, to be an art of medicine. And if it can be proved in any case that human skill has successfully interposed where Nature would assuredly have failed, we have good reason for caution against a general trust in the capabilities of Nature, and a general distrust of those of art. Let us call to mind two or three instances of Art versus Nature. Surgery suggests, among a host of other triumphs, the operation for strangulated hernia, ligatures on arteries in aneurisms and hemorrhages. A man's life is ebbing: he cannot tell why. He looks bloodless—he knows of no hemorrhage—there is no apparent disease. The physician insists on his using a commode for one day instead of a dark water-closet. Florid arterial blood is observed in the evacuations, and, on examination of the rectum, a painless bleeding tubercle is found just within the verge of the anus. The aid of a surgeon is requested for the application of caustic or a ligature, and the patient takes out a new lease of existence. In obstetrics what wonders are wrought by active interference with Nature, e g. in placenta prævia, arm presentations, retained placenta,—nay, craniotomy.

Look at medical treatment in the eye. To say nothing of extracting cataract and making artificial pupils, who has not seen cases in which Nature had been doing nothing, or worse than nothing for months, and yet, on a happy application of some positive disturbing agent, no doubtful invisible medication, but such a thing as lunar caustic or blue-stone, the disease is gone. Or take the skin. A lady suffers from an obstinate lepra—she has long given up medicines—she has been content to mitigate the disease by diet, and baths, and regimen—she has had the disease for years. She is moderately bled—an issue is inserted in the arm, and she takes that triple poison, the hydriodate of arsenic and mercury: in six weeks she is cured. I have chosen for examples, diseases the evidence of which is objective. Neuralgia and other subjective phenomena are open to more doubt.

But to come to the cases in which it is especially needful to have our minds made up as to the right course of practice—I mean the acute visceral inflammations—when I become satisfied that these diseases may be left to pursue their own way, my belief will have suffered a more remarkable *bouleversement* than it has ever yet undergone. We know well enough what these diseases are capable of doing in the way of disorganization, whether meddled with or not. We have met them in the latter predicament in the neglected cases presented to us in dispensary and hospital patients; also in cases of latent inflammation, revealed only by the scalpel—cases in which *Nature* had been stealthily prosecuting her deadly work. But it might, perhaps, be said that there is no sufficient evidence to prove that such cases would have had a better issue had they been placed under orthodox treatment. Some years ago I was called to a patient who had been ill four days. He had been seized suddenly —the symptoms were those of peritonitis. For reasons which it is needless to state, not a single active measure had been employed. He had been confined to bed, taken diluents, and very trifling medicines. When I saw him the belly was tympanitic, with complete constipation and sickness. The pain had lessened, but there was tenderness; the voice had become husky; there was full evidence that the usual effusions had occurred in the peritoneum. I thought the case all but hopeless, but I practised some moderate depletion. Some alleviation ensued, but nothing more. He was not laid prostrate by the remedies, and, as a proof of it, he lived to the seventh day from the attack. He was a strong healthy man of 50. The necropsy showed the extensive adhesions, &c., of sthenic plastic peritonitis. Do you doubt that if, at the onset, the patient had been bled, leeched, and taken opium freely (with or without calomel), he would have had a far better chance of recovery? I have said, "we must act up to our lights." Now, one has repeatedly seen cases begin in a way closely similar, and after, or because of certain remedies, yielding, as to the most essential points, in two or three days—nay, in a day;—the inflammation stopping before fibrin has been effused. I don't believe there can be a greater sceptic in these matters than I am; yet even my Pyrrhonism is overcome when I see the marvellous change induced at the *onset* of an inflammation by a bleeding, a poultice, and an opiate. Even if it be maintained that the *symptoms* only are abated, e. g. the pain and tenderness, and the vomiting and constipation, and the rapid pulse, and hot skin, and scanty secretion, but that still the essential disease, the pathological lesion, the *causa causans*, the *vitiam vitians*, goes on a certain course, and stops only when Nature thinks proper—still I say, thank God for the lancet and opium! My patient is lapped in balmy slumber, and I am quite at ease as to the issue of the malady.

Take another instance. I was called up in the night to a lady, who I found had been ill several days. She had been treated with very mild remedies for what appeared to be hepatic congestion. She was very feverish, and breathed in a way that gave me the impression, when I first set eyes on her, that she was suffering from pneumonia; but I listened to the chest, and failed to find any sign of inflammation within the thorax. The pulse had an irritable character, and there was slight delirium; and, in the absence of proof of local disease, it was decided to try an expectant method, on the probability of the case turning out to be one of fever. The next evening things were rather worse. Again I explored the chest, and this time listened where I *ought* to have listened before, but where I had been deterred by a blister, which had been placed over the region of the liver. There I found crepitation and bronchial respiration. The case was clear. It had been occult pneumonia, which had possibly begun in a part more removed from the surface. Then came a terrible antiphlogistic visitation; all the great guns were fired: the patient recovered perfectly. Relief and abatement of disease ensued at once; but a check was not at once given to the hepatization, which involved two thirds of the lung. The most decided improvement in the signs, both physical and rational, was coincident with mercurial stomatitis. I do not deny that I have known hepatization consequent on neglected pneumonia get well without active treatment; but it has been in a prodigiously longer time than when antiphlogistic remedies had been employed.

Bear with me, if I narrate one more case. A young lady had been ill for many months. For several weeks she had been under a homœopathic practitioner, who was, of course, treating her symptomatically. She got worse, and ordinary medicine was again appealed to. We found an enormous collection of fluid in the left pleura, pushing the heart far over to the right side. We practised very moderate depletion, chiefly with a view to favour absorption; blistered largely; and exhibited diuretics. The case is now under treatment; and, as I have repeatedly seen in like cases, the fluid has been traced receding in proportion as the excretory organs have acted freely, and the discharge has continued from the blistered surface. The heart has resumed its proper habitat, and the symptoms (especially the dyspnœa) are greatly mitigated; but the case will probably end in phthisis.

If one were at a loss for instances in which art interfered with advantage, when Nature was doing nothing, or rather doing mischief, I doubt if any instance could be more striking than the cure and relief of many of the diseases of children by lancing the gums. On the expectant method a child may, after many days of suffering, and after incurring perils in various organs, at last recover when the tooth has pushed its way into the light; but all the pain and jeopardy might have been spared by one or two free incisions.

The efficacy of medical treatment is, I think, evidenced by the short time requisite for the cure of diseases, which, cured by Nature, occupy a far longer period. Within the last six months I have watched two cases of subacute pleuritis, both well marked by the auscultatory signs. In both there were reasons, from the peculiar constitutions of the patients, for avoiding general blood-letting and the stronger medicaments. They both recovered; but I have no doubt, from my experience in similar cases, that a more active treatment, not violent, would have reduced the disease in at least half the time. Whether it would have been ultimately a *gain* might be questioned; but such instances show the power of art over morbid processes.

Time does not allow me to touch on the *reasonableness* of the antiphlogistic treatment—as, for examples, the influence of venesection on the action of the heart, arteries, and capillaries, to say nothing of the blood; the operation of opium on the neurotic element of inflammation—of mercury and antimony on the excretory actions; and the close correspondence between the effects of such agents and the processes which appear to be instrumental in resolving diseases which are cured spontaneously.

But what shall we say of fevers and the exanthemata? Only what nine-tenths of eclectics would affirm, viz. that the less heroically they are treated the better. And why? Because neither theoretical nor empirical therapeutics have suggested remedies that appear to tell on the essential part of the disease, that is, the poison in the blood. At least, I know of none, unless it be chlorine in scarlatina and typhus, when accompanied with putrid symptoms. Yet we cannot but hope that, when the pathology of the blood has reached a development approaching to that of the tissues, we shall have remedies equally direct and appropriate.

I am not fond of arguments from final causes; but can it be doubted that the various medicines we possess were, as such, a part of the plan of the universe, designed to have relation to morbid states of living organisms, as much as esculent matters to healthy conditions? What! were diseases planned? It would seem so; and that it was no less needful that men should depart from the stage of life than they should enter upon it. Disease and death are as *natural* as health and life. And lest it should be objected to this view, that if diseases were intended to sweep men from the earth, antagonists would not be provided for them in the form of medicines, I may remark that this is only one of a multitude of illustrations of the fact that it has pleased Divine Providence to order the world upon a system of checks and counter-checks. Few things show this more strongly than the predatory instincts of one set of animals, and the provisions in a weaker set for eluding and guarding against the attacks of the former.

In your pamphlet, page 31, some excellent reasons are adduced for believing that "Nature can cure diseases without the assistance of art;" but they appear to me to admit of some limitation. Thus it is quite true that diseases are cured "among uncivilized nations, of ancient and modern times, under the sole influence of magic, charms, or other practices equally ineffective;" but, on the other hand, in proof of the inefficiency not only of these methods, but also of Nature herself, one might appeal to the eagerness with which savage tribes invite the medical aid of travellers, and to the extravagant confidence they repose in it, as compared with their anticipations of benefit from their own remedies.

With regard to the expectant system of medicine, though it became very prevalent, we must not forget that its failures generated that disposition to violent heroics which characterised the practice of the first quarter of the nineteenth century. Should the present overweening trust in Nature continue, we may apprehend a similar reaction.

The apparent success of quack medicines, inferred from the large consumption of them, does, I agree with you in thinking, in some measure prove that a vast number of diseases get well spontaneously. Yet we may err in presuming that the popularity of such remedies depends mainly on their seeming utility. Much of the estimation in which they are held may, I suspect, be assigned to the circumstance that a large number of them are, more or less, derived from the class of purgatives; the action of which is always in high favour with the laity. Many a physician has heard the reproaches of patients for the inertness of his prescriptions, simply because they had not produced catharsis—a process associated in their minds with any thing like activity of treatment.

As for hydropathy, the subjects of its milder processes may, perhaps, fairly be said to owe their recovery to the *vis medicatrix;* but in its full administration the system is a glaring instance of *medicina perturbatrix.* It is drugging, and very dangerously, with aqua pura.

That practitioners as they advance in life, become more and more inclined to confide in the resources of Nature, would imply that experience has not taught them to attach as much value to artificial methods of cure as at earlier periods of their career. But we must not lose sight of the fact that age brings not only wisdom, but also an indisposition to enterprise in our own as in other professions. There is also another consideration to be borne in mind. Physicians, whose practice is chiefly consulting, meet with a large proportion of chronic cases; and, as Dr. Alison has judiciously remarked in his essay on Inflammation, in the 'Library of Medicine,' with reference to the value of blood-letting, they are often called in, either when the time has gone by for the remedy to be either safe or advantageous, or when it has already been practised as far as the patient's strength would allow. They are, therefore, less likely to be impressed with its efficacy than those who are present at the onset of the disease, and see it quelled at once by the prompt administration of this measure.

I have not adverted to hygienic methods, which are said to be capable of curing diseases without the aid of other remedies. Allowing this to be true, we must, nevertheless, perceive that they are interpositions of art. If a patient is confined to bed, when but for orders he would be sitting up or walking about; if he is restricted to bread and water, or milk, when otherwise he would be living as usual; if a particular temperature of his room is maintained, &c. &c., and he gets well—his case, surely, ought not to be adduced as an example of the curative powers of Nature. There has been a decided interference of Art.

I should deeply regret were these cursory remarks to give you the impression that I underrate the reparative powers of Nature, or that I advocate any thing approaching to *meddlesome* practice. My intention was to hint, first, that *à priori* we might expect there should be an art of medicine—an art analogous to all other arts; for they all consist in the subjugation of the materials and forces in Nature to the wishes of man; and, secondly, that it is an error to speak of salutary changes, as those of Nature *par excellence*, for she is destructive as well as conservative; exciting processess which tend at one time to disorganization and death, at another to reparation and life; and that it is needful to watch her actions with rigorous attention, nay, with suspicious scrutiny, and to do our best towards encouraging the *vis medicatrix*. He who has learned neither to wait too long, nor to interfere too soon, has made an attainment scarcely inferior to that of knowing *what* remedy should be employed.

I have carefully read your cogitanda; but though there is scarcely a suggestion with which I do not willingly accord, I am of opinion that it is not so much a revolution or reformation of medicine that is wanted, as a more thorough and extensive education of practitioners in the art and science of medicine as it at present stands. The most intelligent and accomplished must sigh for more knowledge in a thousand directions, but the interests of medicine very loudly demand that its professors should *know* what is already *known*. Were pathology pursued and applied, and the operations of remedies observed and made use of, after the methods and rules laid down and illustrated by Dr. Williams, in his 'Principles of Medicine,' and by Dr. Watson, in his 'Lectures on Practice' (I instance these works as *typical* in their respective departments), both the profession and the laity would have practical proofs of improvement in our art, sufficient, I think, to satisfy them that knowledge has been sought in the right tracks. "Our venerable mother," *me judice*, is neither sick nor sorry; nor does she, by reason of age or decrepitude, require a Medea's cauldron in order to come out as "Young Physic." In fact, she is not old enough; she is undergoing development, advancing *towards* maturity, though slowly, and a long way off the goal.

The desiderata are many and great. Not the least is a knowledge of the *natural history* of diseases left to themselves. But who shall furnish this? No one can conscientiously seek it in cases which experience has taught him are capable of arrest or abbreviation by art. Neither you nor I could make up our minds to stand composedly by the bedside of a patient suffering an acute inflammation of the pleura or peritoneum, and in the characters of scientific umpires to say to the sanative and morbid forces, "Now, you antagonist *vires*, we are here to see fair play, a clear stage, and no favour. Our part is not to interfere in your deadly struggle, but to note your relative prowess—*laissez-aller!* Philosophic eyes are on you; and God defend the right!" But as we are not prepared for this position, you ingeniously suggest that we should turn to account the records of homœopathic medicines, as containing reports of maladies that have pursued their course unaltered by remedies. But I fear it is impossible that a physician, under the influence of Hahnemannian doctrines, should observe as *we* should observe; for even minds the most exempt from the bias of preconceived views cannot help, in their observations, seeing more or less the reflex of themselves. An observation is a compound result of the thing observed and the observing mind, the latter being frequently the predominating element. A homœopathic mind could hardly give the same copy of a pathological phenomenon as one that had never succeeded in reconciling itself to the ineffable absurdity involved in the doctrine of infinitesimal doses. The conclusive *reductio ad absurdum*, effected in your article, and in the essay of Dr. Alexander Wood, compels one to suppose that those who have received the doctrine in question have done so in accordance with that strange paradox of Tertullian, "Credo quia impossibile est;" while to us it is a great trial of catholic feeling to forbear exclaiming, when such insults are offered to our reason, "Incredulus odi!" Were other arguments against this fantastical system, with its fractional truth of "similia similibus curantur," and its delectable *psoric* figment, wanted, I think a sufficient one might be found in the improbability that a therapeutical system has a sure foundation, which might be practised quite as well by a person whose knowledge of disease is purely symptomatological, as by one who is profoundly versed in modern pathology. The knowledge which is made up of the coarse or spider-web speculations of other men's minds (and a great deal of medical learning, we may concede, belongs to such a category) may come to nought; but that which is derived from a careful and enlightened observation of natural processes, in health and disease, must sooner or later be susceptible of practical application. But homœopathy has no place for the results of the scalpel, the microscope, and the test-tube.

It has been urged by one very estimable writer, that homœopathy ought to have a fair experimental trial, and that it is unjust to reject the system on *à priori* evidence. But to this I would answer, that life is not long enough for making experiments, unless they are suggested by some antecedent probability. Because a dreamer or dupe tells us that he has a method for extracting sunbeams from cucumbers, are we, in order to show our freedom from prejudice, to waste precious time in putting his nonsensical process to the proof? And because we do not choose to steal the time due to more promising pursuits, are we to be everlastingly reminded of the infamy attached to those who opposed the discoveries of Harvey and Jenner? If, on every occasion that these immortal names have been invoked, a real discovery had been announced to the world, of only one hundredth part of the value which belongs to the doctrine of the circulation and to vaccination, few mortals would become weary of life, and "age, ache, penury, and imprisonment," would become unknown evils. But stars of such magnitude are not born every night. Long ages of darkness must overshadow the world between such portentous births. And yet, if an enthusiast takes it into his head to walk about with a farthing candle, and fancy it "light from Heaven," we are not allowed to smile at his delusion till we have climbed up an observatory designed for watching the very stars, and from so exalted a position taken note of so ridiculous a phenomenon!

Dr. Andrew Combe tells us that we must carefully distinguish the principle of "*similia similibus*" from that of the "infinitesimal doses;" and he suggests that we should test the former separately. But to do this, we must begin an entirely new line of observations. And what is to induce us to administer a scruple of jalap for a looseness, and a grain or two of opium for a lethargy? I suppose Dr. Combe would say, the testimony of homœopathic practitioners. But the observations of these gentlemen on the "like-by-like" doctrine are inextricably mixed up with the alleged effects of infinitesimal doses, and are therefore quite useless to the proposed investigation. Doctor Minimus may aver that he has cured gastritis by decillionths of arsenic, but is Doctor Medius warranted by this testimony in trying to do the same with sixteenths or twentieths? Certainly not. The records of homœopathic medicine give no help to us in solving the question of the sufficiency of the "*similia similibus*" theory separately from the minute doses. Then what *is* to show us the duty of departing from orthodox practice? Is it the statement that Hahnemann persuaded himself that he had seen ague brought on by taking bark? *Quousque tandem?*

O, prophet-sage of Cos! well might you mourn over the brevity of life and the slow progress of art, for you doubtless foresaw that, after more than two thousand years, physicians would fritter away the one and impede the other, in proving or refuting the notions of so whimsical a brain as Samuel Hahnemann's!

But I am myself encroaching on this short life with this long letter; and I fear that after all I have been emphasizing truisms.

Yours, very faithfully,

J. A. Symonds.

II. ON THE PRESENT STATE OF HOMŒOPATHY IN GERMANY.

BY DR. A. MÜHRY, OF HANOVER.

(*Extract of a Letter to Dr. Forbes.*)

. . . . . .

Homœopathy has undoubtedly taught us, that however high an estimate medical men may have before formed of the healing powers of Nature, these must be rated much higher still. This fact may indeed render us more modest, but need not depress us into spiritless unbelievers. If, on the one hand, we ought to feel ashamed that the curative powers of Nature, aided—by nothing at all (ein reelles nichts)—should sometimes achieve as much as we are able to do ourselves, the history of homœopathy has, on the other hand, shown this system to be inadequate to the wants of human society; and has brought into stronger light, not merely the nullities (nichtigkeiten) of medicine, but also as a secondary result, its positive utility.

I think I am right in affirming that the history of homœopathy can be better traced in Germany, where the system originated (fifty years ago), and where its development took place, than in England, where it has not been practised more than from ten to fifteen years. It appears to me, likewise, that the changes wrought in homœopathy have not been sufficiently taken into account in your essay; and hence it may here be of some use to point out some of the principal epochs of the system.

Samuel Hahnemann published his 'Organon' of Medicine in the year 1810, but had begun to make his system known before that time. I think with you that he was a sincere believer in the truth of his doctrine. It is but just, however, to remark, that, at least once previously, he had deceived the world, by selling at a high price, under the name of *pnoeum*, a nostrum which consisted of nothing but *borax*. This is a fact undenied even by his adherents. He had before this published a pharmacological dictionary ('Apotheker Lexicon'), and his system was altogether the offspring rather of Pharmacy than of Medicine, properly so called.

. . . . . .

Among others, the following retractations and retrogressions have been made in the original doctrines of homœopathy, by various adherents. At first it was deemed requisite to administer medicines only once a week or once a fortnight. At present scarcely a single homœopathist adheres to this; but, on the contrary, now frequently orders them several times daily. A contradiction was also recognized in the assumption of a force-creating power (potenzirung) of remedies by attenuation—a greater efficacy in less attenuated remedies. At the present day there is often administered a drop of unattenuated and unpotentialized tincture (termed elementary, or mother tincture [Urtinctür]). Originally, the abstraction of blood, emetics, aperients, mineral waters and baths, were all proscribed: now they are often acknowleged as useful. Hahnemann's later doctrines, as promulgated in his 'Chronic Diseases,' met with few adherents: he had become inconsistent, and became more so still when he recommended camphor in very large, unattenuated doses against cholera. His successors became more and more divided, modifying the system in various ways, until its original meaning was almost entirely lost. This became apparent at a congress of homœopathic physicians, which took place at Magdeburg, either in 1836 or 1838. Their confession of faith attests how much reform and reaction the doctrine of numerous homœopathists had suffered. A fraction of the school has even laid aside the name of homœopathy, substituting for it that of "Specific Medicine." This sect is numerous in South Germany, in the Grand Duchy of Baden. These still hold fast to the principle "similia similibus." as explanatory of the curative process, but they no longer give the remedies after the original method. They busy themselves in experimenting with medicines upon healthy persons, but in large doses, and from this they look forward to grand results, which are, however, not perceptible as yet. They likewise attend to the physiological relations of the diseased body, and they have amongst their body some most able and respectable men. For at least ten years past this "Specific Medicine" has supported a journal.

. . . . . .

We may regard the homœopathists as sincere; but it is true that their number has augmented less by adoption of the original doctrine than of its modifications. They bear the original name wrongly, being no longer true homœopathists. The majority have even resumed a great portion of the allopathic medicines.

I do not hesitate to say that, although men of worth are to be found amongst them, there prevails, generally speaking, both within and without the profession, a low opinion of the standard of their intelligence. In Germany no man of undoubted eminence has ever become a convert to the system. Only once has an instance like that of Professor Henderson occurred: Dr. Kopp, of Hanau, unexpectedly published a volume of 'Practical Observations,' descriptive of a series of homœopathic cases and cures of his own witnessing. He thus stamped himself a homœopathist. A few years afterwards another volume of the 'Observations' appeared, communicating a further series of cases and cures,—but of an allopathic nature. The author thus declared himself reconverted, and he has remained so ever since.

With us in Germany there are in almost every largish town one or more homœopathists, who are perhaps consulted in chronic cases, after various methods had been previously tried, and where it is thought that homœopathy, at any rate, can do no *positive* harm, True it is, that cures are sometimes thus effected, and these create a sensation; whilst the cures accomplished by ordinary medicine pass unnoticed. A large proportion of homœopathists have recently adopted the hydropathic method of treatment. I am not cognizant of any town where homœopathy is exclusively practised.*

. . . . . .

If homœopathy has taught us that our curative control over the course of disease is far below what we rated it at, it has *à fortiori* though unintentionally, taught us that our "apparatus medicaminum" possesses less merit than we imagined, and that it need no longer continue as gross and rude as the instrument of an "armentarium chirurgicum" of old. It has, however, at the same time enabled us to mark with greater distinctness those diseases in which our remedies really effect the cure. Such are, for example, intermittent fever, scabies, syphilis. These it was that first showed the insufficiency of homœopathy; and you will find that in the statistic bulletin of Dr. Fleischmann these diseases are omitted.

One thing certain is that, through means of homœopathy, Medicine has undergone, is undergoing, and—as in your treatise you predict—will, in the future, yet further undergo a rigorous scrutiny. It appears to me, however, that rational scientific medicine has, in some degree already passed this ordeal; and if it can never claim the exactness of chemistry and mechanics, it may yet hope, in point of accuracy, to reach the standard attained by modern Physiology.

Yours, most sincerely,
June, 22d, 1846. A. Mühry, M. D.

---

* I may here mention that, besides the Homœopathic Hospital at Vienna, there is also one at Leipsic, which can, however, with difficulty maintain itself.

III. REPORT ON THE HOMŒOPATHIC TREATMENT OF ACUTE DISEASES IN DR. FLEISCHMANN'S HOSPITAL, VIENNA, DURING THE MONTHS OF MAY, JUNE, AND JULY, 1846.

BY GEORGE W. BALFOUR, M.D. EDIN.

(In a Letter to Dr. Forbes.)

Vienna. August 14th, 1846.

My Dear Sir,—I shall now proceed to lay before you the results of my inquiry into the practice of homœopathy, prefacing them with a short account of its present state in Germany, where it is now become quite fashionable, and nowhere more so than in Austria. Even travelling physician are now chiefly chosen from among its followers, who are, consequently, far from being insignificant in numbers. No young physician settling in Austria, excluding government officers, can hope to make his bread, unless at least prepared to treat homœopathically, if requested; and many, after attempting to do so, return to Vienna to make themselves acquainted with this new method. Many older men also attempt, by thus conforming to the foible of the day, to recruit a failing practice. Thus homœopathy is studied, not for any beauty or truth to be found in its doctrines, but from necessity, for a livelihood. Many continue to practise both methods, not eclectically, but according to the wish of the patient, believing in neither, leaving inquiry to others, and stumbling blindly on Others, confident in homœopathy, merely use ordinary medicine, or, as it is termed, allopathy, in so far as occasionally to give a laxative, or, where the relatives urge it, to bleed—all by way of *placebo*; and I believe there are few, except the older and better established practitioners, who would not give such a *placebo* if requested. Nay, the inclosed recipe will show that even they are not altogether free from blame in this respect, or else that they have found their guiding principle in many cases false. The gentleman who gave me it (an allopathic physician) told me he might have procured many such.

While thus, from force of circumstances, everywhere increasing there domains, homœopathists are far from sitting idly down, content in following the footsteps of their first great master. Imbued with the progressive spirit of the age, they also strive after improvement, and while professing to retain "*similia similibus*" as their fundamental principle, are endeavouring to advance their method, and give it a more permanent and dogmatic character. Seeing, as it would seem, that the above-mentioned principle does not suffice for every case, they change the name of homœopathy to *Specific Medicine*. These men are dissatisfied with Hahnemann's work on the Materia Medica, on account of the imperfect nature of the observations, and look upon it as an enormous and almost unreadable catalogue of symptoms, more fitted for the memory than the intellect, and thus not only rendering the practice of homœopathy more difficult for his own followers, but throwing an almost insurmountable obstacle in the way of physicians who think otherwise, and often preventing those very intellects best fitted to become leaders in the reformed practice from evers tudying it. (Œsterreichische Zeitschrift für Homöopathie, 1 Band, 1 hft. s. 4-5). Accordingly they have commenced a careful re-proving of all the medicines, with the view of obtaining, not a mere catalogue of symptoms, but a collection of medicinal diseases. A journal (the above quoted) is also published in Vienna, with the view of giving publicity to these provings. This shows them to be in earnest in their endeavours to simplify their method and render it more practicable, as any one will be more inclined to confess, after having looked into Jahr's Codex, the best of the day. Who can wonder at the difficulty experienced by homœopathists in choosing their remedies, when we find the same symptoms repeated, it may be, in a somewhat different order, under the head of almost each separate medicine? Nay, Isensee (Geschichte der Medecin, vol. vi, p. 1569) goes so far as to say, that in no case are the peculiar and characteristic operations of a medicine to be found, except in such cases as Hahnemann has, from want of original observation, borrowed from the allopaths; and that his own symptoms may all be referred to sobriety, fasting, ill humour, and sleepiness, caused by continued attention to—*nothing*, mixed with those innumerable sensations which crowd every hour of our life!

Among those who are guided by the same principle in their choice of a remedy, the dose in which this ought to be given has formed, and is forming, a fruitful source of internal contention. Many, amongst whom may be reckoned Fleischmann and most of the Vienna homœopathists, employ the modern formula of 10:90 in making their dilutions, and seldom go higher than the 3d or 4th perhaps, but more rarely the 6th. Others, contending that those dilutions are too low and apt to produce disagreeable consequences, employ from the 14th to the 30th; while yet others, outstripping Hahnemann himself in their idea of the effect of friction and shaking in developing the latent powers of remedies, contend that these are at first produced too powerfully and irrestrainably, and, when employed in those low dilutions, only render the disease worse, without in the least tending to cure; but that by being further diluted (*potenziren* is the technical term) the agent at length arrives at that pitch at which, according to their ideas, the whole remedial power is developed, but so mild and tractable, that it at once cures without producing any exacerbation (verschlimmerung), and is therefore—and then alone—entitled to our confidence and the name of remedy. The champion of this sect is Dr. Grosse, a practising physician, at Jütterbock, whose usual dilutions are 2, 4, 8, and 900; and he often contents himself with allowing the patient to smell the remedy—whether one or more globules at one time I am not aware—waiting patiently for four weeks or so, for the completion of the cure, not even permitting a second smell: so mild, yet certain is the remedial action! This method, however, does not pass current with the homœopaths generally; it is rather too finely drawn for even their sensitive imaginations. A paper by Dr. Grosse, urging the importance of this mode, and accompanied with cases to prove its advantages, was published in the 21st volume of the 'Homœopathic Archives,' and has been criticised in a late number of the 'Austrian Homœopathic Journal,' by Dr. Böhm, a practising homœopathic physician in Vienna, and his cases in proof set aside on the grounds—1st, that in those cases in which a cure seems best made out, lower powers produce equally good results—hence an immense saving of time to the physician; 2d, that many cases recorded as cures seem to be merely a longer than usual intermission between paroxysms, which may, nay, often does occur, without any previous treatment; 3d, that many others were merely the natural result of the lapse of time—e. g. a severe pain in the foot which took its departure four weeks after smelling phosphor. 200; 4th, in others the diagnosis seems to have been incorrect, and the credit due to Nature given to the remedy. Dr. Fleischmann, also, in a note says, with respect to this mode, "Are the results favorable? I grieve that truth appears so decked in folly's garb, as to drive many from her in disgust. Are they imaginary? I grieve again, that spectres arise before which the soberest must retire in fright."*

This Dr. Grosse seems but the pioneer for newer and bolder discoveries in this vast and unknown field; for another writer speaks of arsenic ·2000.

---

* Might not all this be with equal truth applied to homœopathy generally? It is to be wished that common homœopathists would as carefully criticise their own cases, and cease to imagine that "quia post, non propter" refers only to allopathic and high-power cases.—G. W. B.

What would Dr. William Wood say to this, if 30 proved too much for his weak faith and still feebler imagination! At this rate the dilutions of one remedy alone would, with the requisite utensils, form no inconsiderable laboratory, and these preparations consume no trifling portion of valuable time, not to speak of the innumerable trials necessary to be made, in order to find out in which dilution, from ·1 to ·2000, the remedial powers of the agent are most fully and usefully developed; verily, *ars longa, vita brevis.*

There is still another sect, or at least an individual, in Vienna, Dr. Georg Schmid, who has within the last few months given to the world a volume, as the result of many years' experience in homœopathic practice, with respect to the preparation of medicines and the amount of the dose (Ueber die Arzneibereitung und Gabengrösse, Wien, 1846). The latter portion of the work possesses peculiar interest, inasmuch as the author contends, from personal experience, that the present small doses are worse than useless, and that the mother tincture, 1 drop for a dose, or larger doses than usual of the 1st or 2d triturations (verreibungen) ought to be employed. These triturations he employs not because he considers that remedial power or new chemical properties—as solubility—are thereby imparted, but because the medicine being thus divided into its finest atoms, and each particle thus brought into immediate contact with the organism, is better enabled to exert its remedial agency. He adds (p. 60), that by using this means of division, we have not an absolute increase of power, but rather, with the diminished mass, a diminution of it. So long as the smallest trace of the remedy is perceptible, so long can we understand that such a dilution may be efficacious; but when the medicine is neither to be recognised by chemical nor physical means, and probably never will be, how is its efficacy to be explained? His remark upon homœopathic exacerbation deserves to be carefully read by every homœopath. He states that Hahnemann, in his preface to his work on Chronic Diseases, says that nothing worse could happen than that the small doses did not *help*, for *hurt* they could not. "Is it not," asks Schmid, "the duty of a physician to be of positive use to his patient, that is, to help? To do this he must use the proper remedies in the proper doses. But he can also injure; therefore medicine is no child's play, but the life-task of the physician; and he who feels not the power and the courage to discharge this duty, ought to choose some other profession. As there is no means of distinguishing the medicinal disease from the real, so the physician may ascribe an exacerbation which arose after taking his physic, to this cause, though it may be totally unconnected with it; and by then diminishing the dose, may allow this exacerbation to proceed unchecked. How often has not this already happened! Indeed it would not be difficult to discover amongst the many published cases of so-called medicinal exacerbation, merely the progressive increase of the disease, over which the medicine had not the slightest influence, being either of the wrong substance or deficient in quantity; yet the physician has reduced the dose, giving over his patient to nature, it may be to death." (pp. 227, &c.)

Dr. Schmid even turns Hahnemann against himself, showing that of the only four of his cases which have been published, two having been treated with the mother tincture, and two with various dilutions, the first two alone are entitled to the name of cures. Again, Hahnemann, after quoting allopathic cases in favour of his principle, adds, "large doses, though dangerous, often cure without peculiar disavantages." (Organon 4 Aufl. s 54-104.) Finally, Rau—though his work be no longer new—may be quoted as constituting one step towards the formation of an eclectic school; and doubtless, his remarks have afforded much comfort to those homœopaths who have been led astray by interest. He acknowledges that there is much good in the old school, nay, that "an antipathical practice may indeed be thought of." (Organon der Specificischen

Heilkunst, s. 45, 46.) He goes on to say that venesection, derivative and repulsive remedies, may at all times be useful; and quotes cases from his own practice, illustrating the fact that symptoms not to be relieved by homœopathic remedies will often yield at once to purges or emetics.

The Homœopathic Hospital of Vienna is a private one, in the convent of the Sisters of Charity: it was erected by them, in the first instance, with the view of thereby obtaining practical instruction in what constitutes their chief object—the cure of the sick. They have also another smaller hospital in a sister convent in the suburb Leopoldstadt. The homœopathic hospital is situated southwest of the city, in the suburb Gumpendorf, near the lines, much farther from the town than the General Hospital, and upon a loftier and airier situation; the latter being situated on the banks of the brook Alser, which has, within the last year or two, been covered over. The homœopathic hospital is also less than the other, containing only 50 beds—25 for males, and 25 for females, divided amongst four well-lighted and well-aired rooms. A few cribs for children are also occasionally occupied.

From its opening in July 1832, till July 1833, Dr. Mayerhoffer was its medical attendant. During the early part of this period the cholera was ravaging Vienna; and during the prevalence of this disease Dr. Mayerhoffer established his confidence in homœopathy. After him came the above mentioned Dr. Schmid, who treated the patients, indeed, according to the homœopathic principle, but not with the wonted small doses. He continued till January 1835; at which time Dr. Fleischmann commenced those duties which he still continues to discharge, and whose name, in connexion with homœopathy and this hospital, is now known over both the old and the new worlds; and from whom the practice of homœopathy has received a greater impulse than from any other since the days of Hahnemann. During the first appearance of cholera here, the practice of homœopathy was first introduced; and cholera, when it came again, renewed the favorable impulse previously given,—as it was through Dr. Fleischmann's successful treatment of this disease that the restrictive laws were removed, and homœopathists obtained leave to practise and dispense medicines in Austria. Since that time their number has increased more than threefold in Vienna and its provinces.

The medicines employed in this hospital are all prepared in it by lady apothecaries (apothekerinnen), who, for this purpose, receive a special education, and undergo an examination. None of the drugs are obtained from allopathic apothecaries; or, if so, are carefully tested and purified. The tinctures are either made in the hospital, or obtained from those homœopathic physicians who reside where the plants are indigenous. The text-book for the preparation of the tinctures and dilutions is Gruner's 'Homœopathic Pharmacopœia.' Gruner employs the proportions of 10 to 90 in making the latter, or 5 to 95 in the case of such salts, oils, &c., as are not soluble in the above proportion. This dilution is not marked 1, but fractionally, and a double portion 20 : 80, taken to form the second dilution, so as to bring it again into the proper relative proportion. Hahnemann's empirical rules as to rubbings, scrapings, and shakings are discarded. The time employed for the first two is regulated by the relative hardness and adhesiveness of the material; and in making the dilutions the shakings are continued only till the whole is clearly dissolved; when the dilutions are commenced by trituration with sugar of milk, 3 such are made, and one part of the third added to 9 of distilled water to make the fourth. The fifth dilution is made by the addition of one part of the fourth to nine of watered spirit; i. e. spirit containing 70 per cent. alcohol, mixed with an equal quantity of water; and the further dilutions are continued with this spirit. The temperature recommended is 12° to 15° R. The medicines are administered to the patients either in powder or solution, according to the frequency with which they are to be given. For the first method, a drop of the solution, or a grain of the trituration directed, is mixed with a small quantity of sugar of milk for each dose; for the latter, a small bottleful of water is taken, and the trituration dissolved, or the solution dropped into it in the proportion of 2 grains or 2 drops to each ounce, half an ounce being the dose. Those for whom no medicine is considered requisite, get a powder of sugar of milk, or something of that kind. When two medicines are ordered, they are given alternately at intervals of two or three hours. The diet is light and simple; no coffee, tea, or wine is allowed; the latter, however, is sometimes given in cases of old people, or in convalescence from severe disease, if thought requisite. In acute diseases a light soup is given three times a day, exchanged, on commencing recovery, for a more nourishing one (eingekochte suppe), and the diet so progressively improved. No food is allowed to be given during treatment in which acid is predominant.

In taking into consideration the adjuvants to treatment, the religious character of the establishment must not be forgotten. The greater part of the patients are Roman Catholics. These find themselves surrounded by all the consolations of religion—by every thing which, in their opinion, tends to ensure, in the event of death, a speedy passage of the soul to the realms of bliss. Their minds, thus set at ease with respect to futurity, are less gloomy and desponding, and consequently react less unfavourably upon the body than in the opposite circumstances. Nay, looking upon their nurses as self-devoted in the service of heaven and of suffering humanity, they cannot but believe that the blessing of the Almighty will rest upon their labours; and being the object of these labours, they naturally enough appropriate a portion of this blessing to themselves, and imagine that their recovery can hardly fail to be promoted by their being the inmates of such an institution. In support of this opinion, I may state that although most of the patients were young, and many dangerously ill, I never heard one expression indicative of a fear of death, nor one murmur, however slight, unless extorted by the extremest pain, and even then it was more an aspiration after relief than a grieving at suffering. The severer the disease, the more closely do they grasp their rosaries and crucifixes. So long as they are able to read, prayer-books are constantly in their hands, and even in the intervals of delirium, nay, often, in the cases of women especially, during delirium itself, the exercise of repeating prayers, or snatches of them, is their occupation. The superiority of attendance is also one great advantage in favour of this hospital, independently of the important fact just stated, that the nurses are spiritual as well as temporal comforters. It may not be inappropriate to state here also—more particularly as I shall have occasion to compare the results of the homœopathic treatment with those observed in an ordinary hospital—that in Dr. Fleischmann's institution the pneumonic patients are *not* ausculted and percussed, and unceremoniously lectured over several times a day, as is the case in the Genera Hospital. This difference will not appear a matter of indifference to any one who has witnessed the proceedings in the two instances.

The comparative youth of the patients in this hospital must also be taken into consideration. This is at a glance evident to the visitor, and will be rendered so to you, when I state that of 320 patients I have seen treated there during three months, 31 were under 15 years, 90 under 20, 80 under 25, 44 under 30, 22 under 35, 13 under 40, and above 40 were 22, of whom 13 were under 50, 5 under 60, and the remaining 4 were aged as follows—1 of 70, 1 of 73, 1 of 76, and 1 of 95 (the latter died of old age); consequently, above one third of the whole number were under 20 years, and considerably more than one half under 25 years. This circumstance of comparative youth, under all kinds of treatment, has an immense influence upon the ultimate result.

Again, the patients are admitted and discharged by the physician, without any control, so that, to say the least, it requires a man to be very conscientious to decide impartially between temporary improvement and perfect cure, especially when he recollects that the fate of his creed and his institution may depend upon the nature of his returns to government. These returns are made monthly, with a yearly *resumé*.

Some of the following cases will be found to have been discharged too early to enable us to be positive as to the ultimate result. Again, these cases, or others discharged apparently cured, may apply for readmission, and be under some pretext or other, refused; while, to disarm suspicion, a few whose relapses seem more manageable may be readmitted. Such may not be the case in point of fact, still it is very possible. I have seen at least one patient refused admittance, and that, too, the very day after his discharge, without any good obvious reason. It was a boy, with effusion into the right pleura following scarlatina, which he had gone through at home. There was also a general anasarcous state of the body, which speedily disappeared, but the chief complaint remained obstinate, and after 33 days' treatment with bryonia, the 2d dilution, four times daily, he was dismissed but slightly improved. This boy was denied admission when he applied the following day, on account of return of pain in the chest, not certainly for want of room, as his bed was empty for days afterwards. This is not the only effusion into the chest which has been dismissed unimproved during the period of my observations; yet this scarcely agrees with Dr. Fleischmann's returns, as out of 12 with exudation in the pleura, occurring during 10 years, he has, he says, cured all but 3, who died. And a physician of the General Hospital has assured me that many such cases dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief, and which relief they have obtained by the use of purgatives and baths. Then again, there are, I may say, hundreds of trifling cases admitted here, which would not have been admitted into any hospital in England. Many of the patients get no medicine; a few a single dose: and even of comparatively trifling cases many remain for weeks, nay months, in the hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete.

Dr. Fleischmann's usual number of drugs is not very extensive, one drug serving for a great many diseases, but chiefly because the diseases principally consist of a few standard ones constantly repeated. Gastricismus, typhus, and pneumonia are the chief; and in treating these he employs almost always the same remedies, only varying when some one unusual symptom is very predominant. This uniformity is a cause of complaint from his fellow-practitioners, who say that "by seeing his practice you merely get a glimpse of what homœopathy can do; as Dr. Fleischmann, satisfied that his returns are superior to those of any allopathic hospital, gives himself no trouble in trying to suit the remedy to the disease, but is content if occasionally the disease suits the remedy—when it produces those miraculous effects which are the boast of homœopathy."

This may be so: yet it strikes me as being something extraordinary that those cases in which such miraculous effects have been produced, have always been cases of some standing, either ill-treated or not previously treated at all, and the homœopath having come in with his dilutions at the lucky moment when Nature was going to relieve herself. This always puts me in mind of the Irish proverb, "The hour that is darkest is the hour before day." Things could not be worse; they might, and luckily do improve; and the homœopath gets the credit of it. The case is then published as one of the triumphs of homœopathy, whilst the many similar cases where even homœopathic treatment has proved unavailing, are silently passed over, or recorded as instances of the imperfection of the human intellect: the wrong remedy must have been chosen!

Dr. Fleischmann also uses cold applications to the head in delirium, sometimes in headache, and cold washing of the body in fevers; in arthritis, cloths dipped in cold water, and surrounded by oil silk, are wrapped over the affected joints, and allowed to remain so long as they are damp, and are then reapplied. He also uses for costiveness clysters of plain warm water, or mixed with a little salt. In diarrhea, rice clysters are employed. He told me that neither he nor any rational homœopath ever employed emetics or purgatives, however simple; yet I heard his assistant once order a woman to get a spoonful of oil.

The number of students allowed to attend the hospital is limited by the spiritual powers, on account of the nuns. They attend generally in the morning; but during a part of my visit several attended in the afternoon, the number in the morning being full.

The whole process of the admission and discharge of patients is mysterious. Still so much is certain, that most of those admitted have been previously visited at their own houses by the assistant. Many cases not improving, or apparently not likely to improve, are got rid of very summarily. During most of the time, I visited in the morning along with Dr. Fleischmann, and latterly, for some weeks, in the afternoon along with his assistant, it not being then permitted to visit in the morning. I was told the cause of this restriction was that the students might have an opportunity of taking a course from the assistant.

I feel convinced that the secret of Dr. Fleischmann's great seeming success lies in the fact of the admissions and dismissions being entirely uncontrolled, and there being no check on the diagnosis. Rarely other than well-marked cases have their diagnosis written on the board at their bed-head, the others being left blank, and entered in his book, of course, as he pleases.

I have visited the hospital from the 21st of March till the 5th of August daily, except for a short time at first, when I was not in very good health; but I only give you the details of the three months, May, June, and July; as at first I was not sure how I could best conform to your wishes. Three months' regular observations are better than desultory observations extending over a longer period. I have not been able to do all that might have been desired, still I think enough has been done to enable you to judge fairly of the state of things. The patients are in general so stupid that it is not easy even for a native to obtain the requisite particulars from them; of course, it is much more difficult for a foreigner. And as to previous history, they are totally unaccustomed to recollect or relate any thing of the kind. If they have been previously ill, they can seldom tell you how or by whom they have been treated. The restraint belonging to the nature of the institution prevented even Dr. Fleischmann from making that examination which, in general or allopathic practice, at least, would have been thought requisite. Of course, I could do no more; I have, however, done my best to discharge my task faithfully and impartially; and in laying the following statements before you, I may say with Stork—"*Non hypotheses condo, non opiniones vendo, quod vidi scripsi.*"

TYPHUS FEVER.

During the months of May, June, and July, there were treated 32 typhus patients. Four of these still remain under treatment; and, excluding these, six died, giving a mortality of 21·4 per cent. The average age was 21·5.

Under Skoda, in the General Hospital, during the same period, there were 76 typhus patients treated, of whom 15 still remain; 19 of those died, giving a mortality of 31 per cent. The average age was 26·4. Of these patients, however, there were, as I know from actual observation, fewer trivial cases. Skoda's treatment consists in giving acids (mineral), in doses of ℈j or ʒss. daily; extract graminis, or any other equally innocuous extract, by way of *placebo* for the patient; and where the diarrhea is violent, he employs tincture of opium to moderate it, generally in much the same dose as the acids. The following notes, carefully taken at the time, will give you some idea of the homœopathic cases and their treatment by Dr. Fleischmann:

Case I.—J. A., a stout young woman, admitted Tuesday, April 27th. Next day at the visit, stated that for fourteen days past she had been suffering under daily attacks of cold followed by heat; resting, however, well by the night. (*Ipecac. 3d dilution, four times daily.*) On Friday last she was seized with vomiting, which lasted some time; bowels have not been opened for the last eight days; the pulse is accelerated; the tongue coated and dry; skin hot and dry; pain on pressing the abdomen. May 1st. The tongue cracked and bleeding. May 2d. One stool; was restless and wandering during the night; sleeping quietly at the time of the visit; cheeks flushed and eyes sparkling; tongue covered with dry brown scales. 4th. The pain in abdomen is to day more violent and aggravated by slightest pressure. 5th. Bowels opened three times; skin cooler. 6th. Bowels opened eight times; thirst great; sleeps but little; wandering a little at night; the tongue is clearing and moist; the pulse less feverish. 7th. Bowels opened three times; slept better last night; coughs a little to-day, and expectorates a quantity of dull ill-coloured mucus streaked with blood; percussion-sound is good, but sonorous râles are audible over the chest. 8th. Bowels opened six times; abdomen slightly tympanitic. 9th. Her appearance is to-day more lively and sensible; the pain in abdomen better; the diarrhea ceased; tongue moist. 12th. The tongue is again dry and covered with brown scales; bowels not opened for three days; the pulse, however, remains quiet, the skin cool; expectoration continues. 15th. The pain in the abdomen is entirely gone; bowels open to-day. 17th. On account of difficult expectoration she got senega, the second dilution, to be given alternately with the former medicine, which she got on the 28th of April, viz ipecacuanha, the third dilution, four times daily. 19th. She is to-day bathed in perspiration; the pulse quiet; expectoration easy, and tongue again moist and clearing. 24th. Her cough is now much less troublesome. 26th. Her appetite has returned; the expectoration is much less copious, and more easily brought up. 28th. The ipecacuanha stopped, and the cough and expectoration having entirely ceased on the 18th of June, the senega was also stopped. On the 19th of June she was seized with a rheumatic attack in her ankles, which confined her to bed for a few days, after which she continued daily to improve; but was not discharged till the 26th of July.

Case II.—M. M., a delicate-looking-woman, aged 35, admitted on Thursday, April 30th. Complained at visit next day of rigors followed by heat, which together last from six to ten every evening, and have occurred daily for the last three days; they are followed by wakefulness, which lasts till the morning, when she has a little broken slumber; there is constant headache and bitter taste in the mouth; tongue coated; bowels open every third day; no appetite; slight pain in abdomen on pressure. (*Bryonia, 3d dilution, four times daily.*) The rigors left her; the fever increased somewhat, the tongue drying; otherwise her state was unchanged till May 9th, when she was covered with sudamina, having perspired a good deal during the night; the tongue moist and clearing; but on the 13th it was again dry, covered with brown scales; diarrhea set in, the bowels having been twice opened, and continued more or less for three or four days. On the 20th a slight cough came on, with mucous expectoration; this was gone by the 25th; the medicine stopped on the 26th, on which day she was up, still feeling very feeble, but otherwise free from complaint. On the 2d of June she was discharged.

Case III.—T. B., a stout-looking woman, aged 26, admitted Sunday, May 3d. Next day, at visit, found her lying with flushed cheeks and wandering eyes, and a cold compress on her head; she would not speak above a whisper, saying that she was unable to do more; stated that for eight days past she had been suffering from alternate attacks of cold and heat; had no headache nor pain anywhere; was thirsty, and could not sleep; tongue coated and dry; pulse soft and accelerated. (*Belladonna, 4th dilution, three times daily.*) On the 7th she would neither speak nor take medicine, and continued lying in this stupid state till the 9th, when she seemed more sensible; her state otherwise unchanged. 12th. Has had a slight bleeding at the nose; complains of pain in the throat, but nothing can be detected on examination; she also swallows quite easily, sleeps well, speaks rationally; the pulse is quieter; the skin cool and moist; tongue clearing and moist; the feeling in the throat continued for a day or two longer, and then disappeared; she continued improving till the 31st, when she was allowed to rise, the medicine being discontinued; and on the 4th of June she was discharged, pain in the abdomen and diarrhea having been altogether absent.

Case IV.—S. T., a stout woman, aged 26, admitted Wednesday, May 6th. Stated that she had been unwell for eight days. Her tongue is dry and cracked; pulse accelerated; skin cool and moist; no pain in the abdomen. (*Byronia, 3d dilution, four times daily*) 9th. Covered with perspiration; tongue with dry brown scales. 15th. Slight pain in the abdomen on pressure; it is also tympanitic; seemed, however, better; tongue moist; skin cool, and pulse quieter; bowels opened three times; diarrhea continued for four days. On the 21st her cheeks were flushed, pulse fuller and faster; delirium came on during the night. 22d. (*Byronia omitted; opium, 3d dilution, three times daily.*) Tongue dry; skin hot, with an increase of the fever during the night. In this state she continued till the 27th, on which night she died; no section.

Case V.—M. E., a female, aged 31, admitted on Thursday, May 7th; next day at visit, stated that she had been ill for four days with headache and feeling of weakness; her tongue is dry and coated; pulse feverish; pain in abdomen on pressure. (*Belladonna, 3d dilution, four times daily.*) To this was added, in a few days, cough and expectoration of a dark-coloured mucus; and on May 11th, on account of difficulty of expectoration, the belladonna was alternated with senega, the 4th dilution; the percussion-sound was good; the auscultation gave mucous and sonorous râles. On the 17th of May the belladonna was omitted and arsenic given. On the 20th she expectorated a membranaceous mass about the size of a finger; the pulse is quick and weak; countenance rather sunk; face pale and lips blue; skin still hot; delirious at nights. 22d. The senega omitted, and opium, 3d dilution, four times daily, substituted. 23d. Pulse upwards of 130, feeble; thirst great; hands and arms tremble; breathing hurried. 24th. Bowels opened four times; pulse somewhat improved. 28th. Carbo substituted for arsenic; diarrhea continues; alvine discharges black; almost constantly sensible; pulse firmer; appearance somewhat improved. June 2d. The diarrhea has nearly ceased; the countenance again sunk; pulse quick and feeble; expectoration difficult; senega to be given instead of opium. 4th. Bowels still loose; looking better; slept a little. 8th. Sleeps well; cough gone; pulse quiet and stronger. 9th. Skin cool; feels altogether better. 10th. Complains of pain to day for first time; a slough is forming over right hip, which did not, however, separate till the 20th; it was as large as a man's fist. During this time she had considerably improved, but after this she once more relapsed, gradually sinking, and died on the 28th. No section.

Case VI.—K. K., a girl, aged 18, admitted June 15th, stated that she had been ill for eight days, the illness commencing by attacks of cold and heat, accompanied by a continuous headache and pain in the abdomen; has been during this period treated homœopathically; the skin is hot and dry; the tongue dry, brown, and cracked; pulse fast, but rather feeble. (*Arsenic, 4th dilution, three times daily.*) 16th. Has been restless and feverish during night; wandering occasionally in mind; tongue and teeth covered with black sordes; her face is pale; she continued in this low delirious state, with increase of fever at night, discharging her urine and fæces involuntarily, diarrhea having set in on the 20th till the 25th, when her pulse improved, becoming quieter, and her tongue moister. On the 26th she was livelier, and answered questions more readily. 28th. She again relapsed; tongue dry; skin hot; pulse accelerated. June 1st. Pulse weak, scarcely perceptible; respiration hurried; face pale and death-like; four stools. 2d. Skin cool; respiration more regular; tongue moister; pulse quieter; her mind wanders still occasionally; the tongue now began to clear; she lost her wandering, and gradually became more lively in appearance; pulse became quiet, and gradually improved in strength—in short, from this time she steadily improved till the 26th, when she was discharged.

CASE VIII.—M. S., a female, aged 21, admitted July 20th. She was partially delirious, and unable to give any account of herself, except that she had been unwell for three weeks. The tongue is red, cracked, and bleeding; respiration difficult; pulse fast and weak; pain in abdomen on pressure; percussion-sound good; respiration vesicular, mixed with râle; sputa yellow, streaked with blood, and slightly adhering to the vessel. (*Phosphor. 3d dilution, every second hour.*) She gradually sunk, and died on the 26th. No section.

The general run of the cases here resemble the first two or three. Those like the latter seem only admitted occasionally—I suppose by way of attempting a bold stroke. Although I have at present got no more cases written out, yet I have all the severe ones noted, and the period of treatment, which I could furnish you with, if necessary. The average period of treatment was twenty-three days.

FEBRIS GASTRICA.

There were also 23 cases of febris gastrica treated. The average period of treatment was 10·8 days; the average age 27·8; they were all trifling except the following.

CASE.—S. D., a stout boy, aged 15, has been unwell for the last five days; complains of sickness and headache, with a feeling of oppression over the stomach; the tongue is coated but moist; the pulse full, and rather fast; he was admitted on the 17th of May, and got the 3d dilution of nux (vomica?) four times daily. 19th. He has been delirious during night, and this still being the case, on the 20th he got hyoscyamus instead of nux, in the same dose and manner. May 24th. He was asleep; great meteorismus; pulse full and frequent; tongue still moist; skin hot. On the 28th of May the delirium was gone; the pulse quiet, and the skin more natural. He continued after this to improve; the medicine was not, however, stopped till the 11th of June, and he remained in hospital till the 26th of July.

INTERMITTENT FEVER.

There were, during the three months, 41 cases of intermittent fever treated; the average age of the patients being 22·38. All but a very few received medicine at once on entering, without waiting to see whether they had a fever or not; a few had it once slightly, and it never returned. In 7 or 8 it never came on at all, and 30 had it repeatedly, the average number of attacks being 4·7. One had as many as 13; in one case the fever from a tertian became quotidian, and grew so much worse that the patient was discharged at his own request; the remedies employed were china, 2d and 3d dilutions, three or four times daily; ipecacuanha, the 1st dilution, and nux, the 3d, alternately three or four times in the course of the day; and ipecacuanha and nux alone in the 2d dilution more rarely, arsenic, the 4th, or aconite, the 3d dilution. Chinin was only twice employed each time in the second trituration.

Skoda's cases of ague at the General Hospital are treated in the following manner. Each patient, on complaining, gets some extract, centaurii or taraxaci, or some such thing, and one attack is observed—if that come about the time specified, good—the further treatment is proceeded with, if not, a second attack is waited for, so that the time about which the fever may be expected may be known; then supposing it to come at nine o'clock, the patient gets two grains of sulphate of quinine at six, at seven, and at eight —at each hour two grains. The fever, if it be a regular one, may come slightly once after this, but never oftener; so that in regular cases, three times is the oftenest, generally only twice, and in rare and irregular cases four times—never oftener; but it also very frequently remains away altogether under the use of the bitter extracts, even where it has been of six weeks' duration, as I have myself seen; the powders are continued for three or four days after the last attack, in the same manner; a recurrence of the fever during the residence in the wards has not yet been seen, though this residence is sometimes, from the nature of the hospital regulations, protracted.

To return to the Homœopathic Hospital. In one case the fever returned after three weeks' absence, during the girl's residence in the wards; after two attacks, it again left; two of the patients I have seen in the cold fit of the fever, and can testify as to its severity; and two or three I have also seen in the hot fit. A boy who had one of the severest fevers, returned after six weeks' absence, on account of bronchitis; he was looking much better, but as he spoke nothing but Bohemian, I could not interrogate him as to his fever. The following case was also put down as an intermittent:

Case.—W. M., a boy, aged 12, admitted on Sunday, June 28th. Next day, at visit, stated that he had had fever every day for the last four weeks. The attending sister stated that she had observed no cold fit the previous evening, though he had had a hot one. (*China, 2d dilution, four times daily.*) Upon a cross-examination, after Dr. Fleischmann had passed on, he said that for three weeks he had had a regular fever, since then every evening merely a hot fit. The percussion over the heart is dull from the third to between the seventh and eighth ribs. The first sound of the heart is somewhat impure; the pulse is irregular, every five or six beats, one failing, or rather in its place a faltering sort of beat is to be perceived; when lying on his back the heart's pulsations are not perceptible to the touch, though when he sits up, and leans slightly forward, they are then readily felt; this having been found and stated to Dr. Fleischmann, he declared that, on account of the absence of both pain and oppression in the breathing, he could not believe that there was any exudation present, he still believed him to have intermittent. On the 29th and 30th he was stated to have had regular fever, and said himself that he had a slight cold fit; after this he had no more. He was up on the 8th, and discharged on the 9th, the percussion-sound over the heart being unchanged, the first sound purer, and the pulse less distinctly intermittent.

RHEUMATISM.

There have been 27 cases of rheumatism; the average age of the patients was 25. The following deserve notice:

Case I.—M. K., a young woman, aged 20, admitted on the 4th of May. The knuckles of both hands are considerably swollen and red; elbows slightly; complains of pains throughout the body and a pain in the left side; she states that she has suffered from rheumatic fever for fourteen days; there is a slight bruit with the first sound of the heart. (*Spigelia, 3d dilution four times* *daily.*) The arms were after this enveloped in cloths dipped in cold water and covered with oilskin; these were not changed so long as they were damp; continued complaining grievously of pain till the 9th, when a diarrhea set in, after which the swelling and redness of the joints disappeared, and the pain speedily ceased. By the 16th the slight bruit had entirely gone; the joints were quite well; the diarrhea still continuing; the spigelia was stopped and ipecacuanha given; the diarrhea did not return, and on the 23d she was discharged cured.

Case II.—J. P., aged 16, a young man, admitted Tuesday, May 12th. Said that eight days ago he had a rigor followed by a hot fit. Since then his ankles, knuckles, and wrists have been swollen, red, and painful, even on the slightest motion; he complains of pain in left chest, and has a slight cough. (*Acidum phosphoricum*, 3*d dilution, four times daily.*) Upon auscultation a bruit is heard with the diastole, over apex of heart. The rheumatism without further treatment was quite gone by 16th or 17th, leaving, however, a weakness in the ankles, which continued till his discharge; he is altogether a feeble-looking young man; the bruit also gradually diminished, and by the 27th was entirely gone. He was not discharged till the 10th of June.

Case III.—Another articular affection of a similar nature, occurring in a girl aged 18, and admitted on the 10th of June, of five days' standing, was treated solely by the wet compresses, and in five days she was discharged cured.

Case IV.—A. B., a delicate woman, aged 24, admitted on the 25th of June with a general rheumatic affection, of eight days' standing. She had for a year previously suffered under chronic gout and abdominal congestion, as she called it "leberkrankheit;" she continued without any medicine, and without expressing any peculiar feeling of pain till the 15th of July, when she got *Rhododendron*, 3*d, dilution, three times daily.* The pain has settled in the left knee, and almost entirely left the rest of the body; she complained of the most excruciating agony there. and continued to do so till the commencement of August, when I left. The last two or three nights she had not closed an eye, she was rapidly emaciating, and the attending sister said that her knee was much swollen. yet no examination was made.

Case V.—K. B., a healthy-looking young woman, aged 24, admitted June 28th. Stated that a year ago she had had an attack of rheumatic fever, and since then had never been entirely free from occasional twinges; that eight days ago, on exposure to cold, the pains in her knuckles, wrists, and ankles had returned, accompanied by oppression in the chest, and occasional fits of difficult respiration; the affected parts are not the least swollen, indeed she can move them pretty freely; over the heart a bruit is heard at the apex along with the first sound; second sound is natural, but above it in the aorta is heard synchronous with the systole a loud rasping noise; her menstruation is quite correct; she has had a blister on the chest. (*Spigelia*, 3*d dilution, four times daily.*) The fits of dyspnea recurred for four or five days in the evening, and then left; the first sound of the heart became purer, the rasping sound in the aorta still continued; the pulse was generally quiet, and of tolerable strength, but she was much troubled with palpitation, generally recurring about noon. On the 9th of July her hands and ankles were swollen and painful, and continued so till the 13th, when the swelling again vanished; neither the appearance nor disappearance of this had any effect on the general symptoms. About the 29th of July rheumatic headaches made their appearance, and gradually increased in severity till she was discharged on the 3d of August. The sound in the aorta the same as before. The first sound still impure, the ankles weak and painful, the palpitations recurring daily.

The following case had also "Rheumatism" written as its diagnosis:

Case VI.—J. P., a young man, aged 19, complains of pain along the region of the spine; the inferior extremities are nearly completely paralysed; whenever he attempts to move them he only produces slight convulsive twitches; an enlarged gland is situated in the right submaxillary region. (*Dulcamara, 3d dilution, twice daily.*) The urine flowed off spontaneously; the bladder on percussion is found to be quite distended, and in this state it remained during his residence. On the 28th of June the dulcamara was omitted, and *nux, 1st dilution, four times daily* given. He had two or three attacks of intermittent, and ipecacuanha was added on the 31st; this did not recur. On the 1st of July, having had no motion for eight days, and the abdomen being quite tympanitic, a clyster was given. After this nothing occurred; he gradually sunk and died on the 28th of July, sores having formed on his œdematous feet.

The other cases of rheumatism were pleurodynia, or slight muscular pains. The two following cases remained in the hospital when I left:

Case VII.—A girl, aged 16, admitted on the 31st of July. She had been ill with rheumatic fever for three weeks. The right elbow and shoulder and left ankle were swollen, red, and painful; there was no heart affection. She got aconite, 2d dilution, every second hour.

Case VIII —A girl, A. F., aged 23, and admitted on the 27th of July, had been ill five days with a rheumatic attack in the muscles of the neck, and subject to daily exacerbation in the afternoon. However, on the afternoon of her admission this had not taken place, consequently could not be attributed to medicine, which she had not got. She improved steadily, and though well was not discharged when I left. She had aconite, 3d dilution, four times daily.

DIARRHEA.

There were eleven cases of simple diarrhea treated: average age 26·3. The medicines employed were acidum phosphoricum, 2d and 3d dilutions; ipecacuanha, 3d dilution, four times daily. In no case was the diarrhea checked or even moderated on giving the medicine, but in the course of a few days it gradually ceased, though frequently with previous exacerbations.

DYSENTERY.

Three cases of dysentery occurred.

Case I.—A female, aged 31, admitted on Friday, May 8th. Has been unwell for eight days; yesterday the stools were, for the first time, accompanied by blood and straining; no thirst nor pain in the abdomen; tongue coated but moist; pulse very little accelerated. (*Sublimat., 3d dilution, four times daily.*) The stools continued five or six times daily, mixed with blood and mucus till the 12th of May, when they became more natural; ceased altogether on the 13th. The medicine was stopped on the 15th, and she was discharged on the 18th.

Case II.—A man, aged 28, who had been unwell for three or four days; his stools, on admission, the 29th of April, were chiefly blood and mucus, accompanied by straining and pain in abdomen on pressure; they amounted to 40 in the course of the day. (*Sublimat., the 3d dilution, four times daily*) His pulse is slightly accelerated; skin natural; tongue moist; the fever increased slightly for a day or two, the skin becoming hot, and the tongue dry, and again relaxed, both returning to their natural condition. On the 7th of May the stools had diminished to eighteen. (*Sublimat.* removed, and *rhus, 3d dilution, three times daily,* substituted; to which *ipecacuanha, the 2d dilution, four times daily,* was added on the 9th. By the 12th the stools were more natural, and by the 14th had entirely ceased. He remained in hospital, having been much reduced, till the 4th of June, when he was discharged.

Case III.—A. S., a woman, aged 45, admitted July 14th. Stated that for eight days she had been afflicted with bowel complaint, having had thirty stools in the course of each day. Each stool consisted of watery mucus, mixed with blood, and was accompanied by straining and griping. She has headache, feverishness, and sweats much, sleeping little; the pulse is full and accelerated; the tongue coated and moist. (*Sublimat., 3d dilution, every third hour.*) 15th. Has had only one stool since admission, and that one without blood, free from pain, and more natural; slight pain in abdomen on pressure. Her bowels were not again opened till the day before dismissal, when she had a natural stool; the pain in abdomen had entirely disappeared. She was dismissed on the 20th of July.

CHOLERA.

Two cases resembling cholera sporadica were treated.

Case I.—F. F, a man, aged 26, admitted Sunday, July 12th. Has always previously been healthy till Tuesday last, after eating a "saures rindfleisch," which is meat two or three days after its first cooking, rewarmed with a sour sauce, and which, I have been assured, very frequently produces the same symptoms, being a slight poison. Since then he has been almost constantly purging and vomiting; has vomited twice to-day greenish matter; stools to-day have been seven, watery, and forcibly expelled; he has headache, pain in the abdomen, cramp in the hands and feet; frequent eructations and great thirst; the tongue is red and dry; pulse fast and feeble; voice low and faint. (*Veratrum, 2d dilution, every second hour.*) 13th. Feels better; has not vomited; only twice had stools since yesterday; two attacks of cramp in the hands during night; his pulse is a little fuller, and the voice improved. 14th. Bowels again twice opened; no cramps; tongue moist, clean; pulse full and quiet; appetite returned. 15th. No pain anywhere, nor cramps; bowels opened twice. 17th. Medicine omitted, and on the 20th he was discharged.

The second case, with somewhat similar symptoms, was admitted on the 31st of July, and was left in treatment. The patient (a woman) was not, however, improving so rapidly.

COLICA.

There were three cases of colic.

Case I.—This was a simple colic; ceased in a few days under the use of *opium, the 6th dilution, three times daily.*

Case II.—This a colica menstrualis. The patient got *cocculus, 6th dilution, twice daily;* the third day became furiously delirious, for which she got *stramonium, 3d dilution, four times daily,* and was then removed into a private room, after which I never saw her.

Case III.—H. B., a stout man, of 42, admitted Thursday, July 16th. He stated that he had been for years a painter; had previously had colic four or five times, for which he had been treated in the General Hospital here, and this was his first trial of homœopathy. He had intermitting pain in the abdomen, twisting about the umbilicus, and relieved by pressure; obstinate constipation; cramps in the calves of the legs and arms; loss of power in the hands; the edge of the gums where they join the teeth is of a blue colour; pulse quiet. (*Opium, 1st dilution, four times daily.*) 17th. During the night he got a clyster, which brought away some fæcal matter, and he is now easier. 18th. The pain has returned somewhat; he again got a clyster, which again brought away some fæcal matter, and afforded some relief. On the 20th the attack was again worse, but it had got better spontaneously by the 21st. He says himself that it is his worst attack, and that he has been more speedily relieved than at any former period. After this the pain and cramps remained entirely away, his hands were restored to their wonted condition, but his bowels were not opened since the 18th till the 27th, when they spontaneously and copiously relieved themselves. He was discharged on the 28th.

SCARLATINA.

Two cases of scarlatina were treated.

Case I.—J. R., aged 12, admitted July 9th. The next day at visit, he was lying with his eyes shut, in a half comatose state; was stated to have been very restless during the night, and was indeed bound to the bed. On being aroused, which he readily was, though he speedily relapsed, he stated that since Sunday he had been unwell with pain in the abdomen and throat. His whole skin is of a bright scarlet colour, which disappears on pressure, returning from the circumference; his tongue red and dry; throat is scarcely to be seen, on account of his unwillingness to open his mouth, and when it is forced open he complains of pain most grievously; it is internally much swollen and red; no swelling externally; he has had a few leeches on his throat; pulse very feverish; skin hot. (*Belladonna, 1st dilution, every second hour.*) 11th. An eruption like sudamina is scattered over the chest; he has been more restless than ever during the night; the comatose state still continued. The fever increasing, he died on the evening of the 12th. No section.

Case II.—J K, a boy, aged 7, admitted Sunday, July 26th. Next day, at visit, he complained of sore throat, but was unable to state how long he had been ill. Skin covered with a red rash, which disappears on pressure, returning from the circumference, and in points; he has on a thick cotton night-shirt, beneath that several cloths; his hands and feet enveloped in stockings, so that nothing but his face is left uncovered; he will not put out his tongue; his pulse is very slightly accelerated, for a boy of his age perhaps not at all. (*Belladonna, 3d dilution, every third hour.*) 29th. Tongue red and dryish; eruption beginning to fade. 30th. The cuticle beginning to scale off; his pulse is full and slow. 31st. Sitting up; a slight ulcer has formed on the tip of the tongue; medicine discontinued; his stockings, &c. are discarded; I left him running about the ward quite well.

PNEUMONIA.

Case I.—F. H., a stout-looking young man, aged 22, admitted on the 5th of May. Next day, at visit, stated that five days previously, after a sudden cooling, he had been attacked by alternating fits of cold and heat, followed by pain in the chest and cough; the two latter symptoms have since been constant, though the cough has not been very troublesome; he has, however, during the night expectorated a considerable quantity of a dark bloody-looking fluid, which still continues to be brought up in less quantity, mixed with mucus and saliva; his countenance is extremely anxious; his respiration hurried and imperfectly performed; his pulse full, but extremely rapid; on percussion the sound on the right side anteriorly is good as low down as the centre of the mammary space; on the left the same, with the exception of a space of two and a half inches from the left edge of the sternum and from the fourth rib; this space, and all beneath on both sides, are dull; this dullness posteriorly extends as high as the centre of the scapula and intrascapular spaces on both sides; the respiration on both sides, over the dull portions, is pure bronchial; over the superior portions of both lungs vesicular; though, from the above-mentioned state of the respiration, not always very distinct. (*The 3d dilution of phosphorus was ordered to be given every second hour.*) The patient died on the afternoon of the 6th.

Section, Friday, 8th, 10 a.m.—On opening the thorax both lungs were found firmly connected by cellular adhesions to the parietal pleura, particularly their inferior lobes; over the superior and external portion of the right lung was a small, soft, albuminous-looking exudation, of about half an inch thick; the inferior lobe of the left lung and the inferior and middle lobes of the right one were in a state of red hepatization, soft, and easily broken down; a considerable quantity of reddish serosity gushed from their cut surfaces; the superior lobes of both lungs contained a quantity of frothy mucus; the cavities of the heart and large vessels contained a small soft clot; the intestines were gorged with blood, and the glands of the ileum slightly swollen. The other organs were not examined, except the spleen, which was soft, but scarcely enlarged.

Case II.—F. H., a healthy-looking boy, of 16 years, admitted on the morning of Saturday, May 9th. At visit, same day, stated that two days ago he had a rigor followed by a hot fit, accompanied by perspiration, and followed by pain in the left breast and cough, which were, however, so slight that he came in only on account of the feverishness which still continued. He has always previously been healthy, and never subject to cough; his cheeks are flushed; tongue somewhat coated, but moist; pulse full and accelerated; sputa frothy mucus streaked with blood; percussion-sound normal; respiration vesicular, marked, however, on the posterior portion of the left side, from the lower third of the scapular space downwards by a fine crepitating râle. (*Aconite, the 4th dilution, every third hour.*) 10th. Where crepitation was yesterday audible, bronchial respiration is to day to be heard; the percussion-sound is also over the same space dull, anteriorly; from the lower portion of the mammary space downwards on the left side slightly tympanitic, otherwise normal; the sputa are now rusty, and slightly adhesive. 11th. The pulse is to-day much quieter; the physical signs remain the same. This state continued till the 13th, when over the superior portion of the dorsal space a crepitating râle was audible, accompanied by bronchial expiration; in the lower portion of the scapular space the respiration is still pure bronchial; dullness on percussion the same; right side normal in every respect. 14th. The dullness on percussion is to-day but slightly perceptible; the respiration is everywhere vesicular; over that portion of the left lung where bronchial respiration was formerly heard it is a little rougher than elsewhere, and accompanied by an occasional mucous râle. He has been a little restless and wandering in mind a little during the night; quite calm and collected now; has also had a slight bleeding from the nose; expectoration mucous; skin cool; pulse quiet and regular; tongue moist and clean. May 15th. Has again had a slight bleeding from the nose; percussion-sound everywhere normal; auscultation gave vesicular respiration, with a few mucous râles; the cough pains him slightly, and he has a general feeling of soreness over the chest; appetite has returned; his expectoration is mucous, and but slight; he continued improving, the râles not having been again heard; and on the 18th was allowed to get up, and discharged on the 21st.

Case III.—F. D., a slightly-made boy of 16, with a clear skin and delicate complexion, admitted on Saturday, May 16th. Stated that he had been unwell and feverish since the Thursday previous, with slight cough and pain in the chest; he had formerly been subject to cough; percussion-sound over the right lung posteriorly was dull as far down as the centre of scapular space; anteriorly over the same lung tympanitic, as low as the commencement of the mammary region; over the rest of the chest normal; the respiration was found on auscultation everywhere vesicular, with the exception of the above-mentioned dull space, where it was bronchial; tongue moist and coated; pulse full and accelerated. (*The 4th dilution of phosphorus to be given every third hour.*) In this state he continued, the pulse remaining firm and full; the skin hot and dry, and the inflammation progressing daily till the 20th; when the whole of the right side posteriorly was found dull on percussion, and the upper portion of the left, as low down as the middle of the scapular space; anteriorly, over the entire right side, the sound is tympanitic; over the left side good, with the exception of the subclavicular space, where it was somewhat duller. On auscultation, pure bronchial respiration was heard over the entire right side posteriorly, and the superior portion of the left side posteriorly, elsewhere, the respiration was vesicular, and, though somewhat quickened, not laboured; the sputa were rusty and adhesive; the skin hot and dry; the tongue coated and dry; pulse full and bounding. 21st. On the left side, posteriorly, the dulness and bronchial respiration to-day extends as far down as the lower third of the scapular space; the other physical signs remain unchanged; the skin is cool; pulse quiet, and the tongue moist. 22d. The percussion remaining the same; the sounds, on auscultation, are so far altered, that over the acromial and upper third of the scapular space, on the right side, instead of pure bronchial, an undecided respiration is heard. May 23d. The physical signs the same as yesterday, with the addition of a slight crepitating râle occasionally audible over the right acromial and upper portion of the scapular region; the patient is looking livelier; his skin cool; pulse quiet; expectoration looser, and more catarrhal. 24th. Percussion as formerly; the respiration over the entire right side, posteriorly, was sharp, vesicular, puerile, mingled with occasional fine crepitating râles; anteriorly, on both sides, vesicular; posteriorly, on the left, undecided; marked bronchial as low as the top of the middle third of scapular space; the middle third bronchial; beneath vesicular; expectoration colourless mucus. 25th. Vesicular respiration every where to be heard, a little rougher in character over the middle third of scapular space of the left side; where the bronchial respiration was yesterday audible, no râle is to-day audible. 26th. Physical signs as yesterday; the dulness on the left side perhaps a little less distinct, and a few subcrepitant râles are heard over the right posterior dorsal space; cough is not so troublesome; expectoration easy, mucous. 27th. Left acromial and scapular regions decidedly clearer; no râle audible; respiration every where vesicular. 28th. Entire left side clear on percussion; otherwise no change. 29th. No change. 30th. Dulness over right side; posteriorly, less in intensity; anteriorly, normal; left side normal; numerous mucous râles are to-day mixed with the vesicular respiration; the expectoration and cough are somewhat increased. (*Medicine to be given only thrice a day.*) The patient continued daily to improve, and on the 4th of June there was only an occasional mucous râle audible over the right dorsal region; and on the 6th full, clear, vesicular respiration every where audible; the chest expanding well, and the cough gone; but still the percussion slightly dull, though improved, on the right side posteriorly; after this, the patient was always up and dressed during visit, and was discharged on the 11th of June.

Case IV.—L. U., a stout healthy-looking young man, aged 18, admitted on Saturday, the 16th of May. Stated at visit, next day, that he had been on the previous Wednesday seized with pain in the chest and cough, which continue. Percussion-sound anteriorly over left side normal; over right side tympanitic, as low as commencement of mammary space; posteriorly, left side, normal; right side dull down to centre of scapular space; beneath normal; respiration every where vesicular, except over dull space on the right side posteriorly, where it was bronchial; sputa rusty, adhesive; pulse quick and full. (*The 3d dilution of phosphorus to be given four times daily.*) May 18th. Physical signs the same; skin covered with profuse perspiration; bowels opened six times. 19th. Bowels to-day not opened; on auscultation, where the bronchial respiration was audible, it is now indistinct and mixed with a fine crepitating râle; pulse quiet and regular; expectoration mucous. 20th. Respiration to-day every where vesicular; the crepitant râle still audible in the same situation as yesterday. 21st. The percussion-sound over affected part is to-day somewhat clearer; condition otherwise the same. 22d. Percussion-sound still improving, and on the 23d every where good; the vesicular respiration is every where audible, and free from admixture of râle; cough gone; after this the patient remained in hospital free from all complaint till the 28th, when he was discharged.

CASE V.—E. B., a stout-looking woman, aged 48, admitted Saturday, May 16th, afflicted with pain in chest and difficulty of breathing She got *the 3d dilution of aconite, four times daily;* exchanged on the 20th for *the 3d dilution of phosphor., four times daily,* and being on the 21st evidently much worse, mind excited and wandering; expectoration retained. *Tartarus emeticus* was given in the 2d dilution, three times daily. The patient died during the day.

SECTION, Saturday, May 23d, 10 A.M.—Extensive plastic exudation was found between the visceral and parietal portions of left pleura, forming at the lower portion a sac containing fluid; the entire inferior lobe of left lung in a state of gray hepatization; right lung and upper lobe of left, though not to the same extent, gorged with blood and serum; bronchi filled with frothy mucus; right lung connected to costal pleura by old cellular adhesions; a small amount of plastic exudation in the pericardium; the right heart contained a fibrous clot, the left one was empty; other viscera, so far as examined, normal.

CASE VI.—A. T., a stout-looking man, aged 46, admitted Thursday, May 21st. Same day, at visit, stated that he had caught cold, and had now great pain in breathing, accompanied by a slight cough; he has not expectorated since admission; percussion every where normal; respiration vesicular; pulse full, strong, and accelerated. (*Aconite, the 3d dilution four times daily.*) May 22d. Expectoration rusty, blood-stained; percussion anteriorly on left side normal; on right side dull as high as centre of mammary space; posteriorly, on left side, normal; on right side dull as high as centre of scapular space; dulness likewise extends round over the lateral and infralateral spaces; elsewhere vesicular; pulse still full and bounding; *aconite* stopped, and *phosphor.* given, the 3d dilution, four times daily. 23d. Not examined, on account of his evidently dying condition; the breathing hurriedly and imperfectly performed; the countenance extremely anxious; pulse still more accelerated, but failing in firmness and strengly. He died in the course of the day. No section was made.

CASE VII.—F. B., a young woman, aged 24, admitted Sunday, May 24th. Next day, at visit, she stated that, having been always previously healthy, she had five days ago caught cold, and since then been suffering from pains in the chest, accompanied by cough, most troublesome at night. Expectoration slight, rusty, and adherent, has no headache, and no appetite, but great thirst; percussion-sound good, except posteriorly over right lung, as far down as centre of scapular space; beneath this again good; on auscultation, mucous râles are heard pretty generally distributed over both lungs; over the dull space bronchial respiration is audible; pulse accelerated; tongue coated, inclined to be dry. (*Phosphor. 2d dilution, three times daily.*) 26th. Physical signs the same; the râles, however, are not audible to-day; the tongue is moister, pulse quieter. 27th. The expectoration more mucous, less adhesive, still blood-stained; respiration undecided over dull portion. 28th. Respiration every where vesicular; over the dull portion rougher; more puerile; expectoration mucous; the dulness is also a shade better. After this she continued improving; by the 31st her cough was entirely gone; the medicine was, however, not discontinued till the 2d of June, and she was discharged on the 4th with still a shade of dulness over the right side, posteriorly extending, however, no farther than the top of the scapular space.

CASE VIII.—A. S., a young man, aged 17, admitted Wednesday, May 27th. Next day, at visit, stated that the previous Sunday he had fallen suddenly to the ground without known cause; had always previously been healthy. Has since then been troubled with headache, sleeplessness, slight cough, and pain in the chest; he feels weak, and is tormented by thirst; his tongue is red, but moist; skin natural, and pulse but slightly accelerated; the percussion-sound is anteriorly normal, with the exception of over the right clavicle, where it is slightly dull, and where a fine mucous râle is to be heard; posteriorly, on the left side, normal; on the right one also good, down to nearly the inferior border of the scapular space, beneath which it is dull; the respiration is vesicular, obscured posteriorly on the right side by loud, sonorous, and sibilant râles; expectoration, a jelly-like mucus. (*Phosphor., 4th dilution, every third hour.*) 29th. Face to-day has a dingy appearance, with a dusky red spot on each cheek; pulse more accelerated; sputa adherent, of a deep orange colour; percussion as yesterday; pure bronchial respiration over the posterior inferior portion of the right chest, mentioned yesterday as dull; superiorly, still an occasional sonorous râle to be heard. 30th. The dulness on percussion extends now half across the inferior portion of the lateral and infralateral spaces; anteriorly, the right infra-mammary and inferior portion of the mammary are tympanitic, the rest normal; bronchial respiration is audible over the dull portions; sonorous râles are now more audible over the right anterior portion of the chest. 31st. Posteriorly, the percussion-sound remains the same; the portion anteriorly, which gave yesterday a tympanitic sound, gives to-day a slightly dull sound; where bronchial respiration was yesterday audible, the respiration is to-day indistinct, and accompanied by consonating râle. June 1st. The percussion remaining the same, a fine, loud, consonating râle is heard over the dull spaces, both anteriorly and posteriorly on the right side; the left side still gives only vesicular respiration; the skin is still dingy; the sputa less yellow and more copious; the pulse quieter. 2d. The percussion unaltered; the râles less frequent; the expectoration more copious, and now of gray mucus; the patient sleeps better, and the colour of his skin has decidedly improved. 3d. Percussion, posteriorly and laterally, as formerly; anteriorly normal, except over right clavicle, where dulness still continues; auscultation as formerly; expectoration grayish mucus; tongue moist and natural; skin cool, pulse quiet. 4th. A few consonating râles still to be heard, mixed with an undecided respiration. 6th. Percussion good, excepting, as before, over the right clavicle, and also the right infralateral and inferior portion of lateral region, where it is still a little dull; the respiration is also there undecided, elsewhere vesicular; somewhat puerile over the posterior inferior portion of right chest. On the 8th, the percussion and respiration being everywhere normal, except over right clavicle, where slight dulness still continues; the medicine was discontinued; the expectoration slight; cough almost gone. On the 10th he was up, and on the 15th discharged, both having entirely ceased.

Case IX.—M. U., a young woman, aged 19, admitted on Thursday, May 28th. Stated next day, at visit, that on the Sunday previous she had been seized with shivering, followed by heat, pain in the chest, and cough. Has been previously subject to cough; expectoration tolerably copious, adherent, and blood-stained; percussion normal, except posteriorly, on the right side, where it is dull as low down as the centre of the scapular space; beneath that normal, over the dull portion bronchial respiration is heard; over the rest of the chest sonorous and sibilant râles. (*Phosphor. the 2d dilution, to be given every second hour.*) 30th. Expectoration more copious; cough more troublesome; physical signs unchanged. 31st. Breathing laboured and anxious; entire right side, posteriorly, dull; anteriorly also; percussion-sound less clear than on the left; posteriorly, over the right chest, bronchial respiration was heard, mixed with consonating râle; over the left side, sonorous and sibilant râles; anteriorly, over both sides, loud, sonorous, and sibilant râles to be heard; expectoration still copious; pulse full and accelerated. June 1st. Physical signs unchanged; breathing less oppressed, and her look less anxious. 2d. The râles are to-day not so frequent. 3d. They are to-day much louder, especially anteriorly, and quite perceptible to the touch, i. e. the thrill caused by them is so; and the respiration is again more laboured; the pulse is, however, not so feverish. 4th and 5th. Her condition continued to improve during these two days; the expectoration becoming more copious, and more free from blood-stain; and on the 6th, there were anteriorly no more râles to be heard; the percussion, anteriorly, is normal; posteriorly, the right side is still dull; the respiration there is, superiorly, undecided; inferiorly, disguised by sonorous râles; a few of these are also to be heard over the left chest posteriorly; the expectoration is copious, gray frothy mucus; the cough is troublesome. (*Medicine to be given only twice daily.*) 7th. Except an improvement in the percussion-sound of right chest, her state is unaltered. 8th. Percussion everywhere good; respiration vesicular; disguised, posteriorly, on the right side, by sonorous râles. 10th. Vesicular murmur to-day everywhere audible; cough greatly better; expectoration more easily brought up, less copious. On the 11th she was allowed to rise; and was discharged on the 13th, her cough having entirely ceased.

Case X.—M. B., a stout healthy-looking man, aged 58, admitted Tuesday, June 9th. On the morning of the 10th he was going about the ward, and was not noticed till the end of the visit, when there was no time to examine him; no medicine was ordered. On the 11th he stated that he had come in the instant he felt himself unwell; he complained of pain in the chest and cough; the expectoration copious, adherent, of an orange tint; pulse full, but slightly accelerated; percussion, anteriorly, over the left side, tympanitic; over the right normal; posteriorly, the entire left side dull; the right one normal; respiration vesicular, except posteriorly, over the left side, where nothing is audible but a loud consonating râle. (*Phosphor. 2d dilution, every third hour.*) This state remained unchanged till the morning of the 15th, when the percussion-sound remaining the same; instead of the consonating râle formerly heard, a fine crepitating one is to-day audible over entire posterior surface of left chest; the expectoration is copious, mucous; the pain very much better. 16th. The respiration is to-day vesicular, a few crepitating râles being still audible over their yesterday's seat; the percussion-sound is also less manifestly dull; the expectoration less copious, more easily brought up, and cough less troublesome. He gradually improved till the 21st, when the percussion-sound was normal, and the respiration vesicular over the entire chest; a full inspiration still, however, produces a subcrepitating râle over the former seat of disease; and this continued to be the case till the 2d of July, the patient having been up and going about since the 22d of June: on the 5th of July he was discharged.

Case XI.—W. B., a boy of 15 years of age, admitted Friday, June 12th. On the 13th, at visit, stated that for four days he had been troubled with a cough and severe pain in the left side. Expectoration rusty and adherent; pulse but little accelerated; the percussion-sound normal, except at the under portion of left chest, where it is dull anteriorly as high as the centre of the mammary region, and posteriorly as high as the centre of scapular region; the dulness extends entirely round, through the lateral and infralateral regions; the respiration is over the dull portion bronchial, elsewhere vesicular; the heart's tones are clear and unaltered. (*Phosphor. 3d dilution, every third hour.*) His condition remained unchanged, his nose having bled slightly on the 16th till the 17th, when, the percussion being the same, the respiration is over the dull portion undecided, mixed with mucous râle; bronchophony still continues; expectoration mucous. 18th. Again a bleeding from the nose during night. 19th. The percussion-sound is to-day somewhat clearer; mucous râle still audible, and, accompanying the respiratory movements, a fine rasping sound is to-day for the first time audible, about the lower edge of the scapular space. 20th. The rasping sound is to-day almost inaudible; the respiration vesicular, with a few mucous râles; the percussion is everywhere normal: the expectoration trifling, and the cough almost gone; he is anxious to get up. 21st. Up and dressed; chest expanding fully, and without pain. On the 25th the cough having entirely disappeared, he was discharged.

Case XII.—A. S., a delicate-looking young man, aged 25 years, admitted Sunday, June 12th. Next day, at visit, stated that for the last day or two he had lost his appetite; had pain in the left side; feverish pulse; no cough; tongue coated, and a slight griping diarrhea, bowels having been opened three or four times daily; he had been treated in this hospital December previous for pneumonia; the percussion was normal; as also the respiration. (*Ipecacuanha*, 3*d dilution, four times daily.*) The same afternoon, pneumonia having manifested itself, he got from the assistant, *Phosphor.* 3*d dilution, every third hour*, the preceding medicine being discontinued. 14th and 15th. No visit, in consequence of religious feasts. 16th. Expectoration copious, partly rusty, partly of an orange tint; percussion every where normal, except posteriorly over the inferior portion of the left chest, where it is dull as high as the inferior border of the scapular region; over this dull portion the respiration bronchial, elsewhere vesicular; pulse feverish; skin hot and dry; respiration somewhat laboured. 17th. The percussion remaining the same; the respiration is to-day undecided; the pulse quieter; the expectoration more mucous, blood-streaked; the breathing more easily performed. 18th. The dulness to-day extends as high as the centre of the scapular region on the left posterior surface of the chest; elsewhere it is normal; over the superior portion of this dull space the respiration is undecided for a short distance down, perhaps the distance of two ribs with the intervening space; next to this comes a layer of pure bronchial respiration of equal extent, and beneath, the respiration becomes again undecided; over the other parts of the chest the respiration is vesicular; the sputa are mucous, and more easily brought up; cough less troublesome, and unattended with pain. 19th. The percussion the same; over the superior portion of left dorsal region a fine crepitating râle is audible; immediately above this a layer of undecided respiration, and above this, up to the centre of left scapular region, the respiration is bronchial; elsewhere vesicular. The crepitation gradually progressed over the diseased portion, till by the 23d it occupied it entirely; the sound, on percussion, had also gradually improved, and was now almost normal. On the 24th, he was up and dressed; cough infrequent; still feels a slight uneasiness in left chest, but is able to inspire fully without pain. On the 27th he was discharged.

Case XIII.—A. L., a stout boy of 14 years, admitted on Sunday, June 28th. Next day, at visit, stated that he had caught cold on the previous Friday, after bathing, and on the Saturday felt himself unwell, with pain in the chest and cough, to which he had never been subject; the expectoration is rusty and adherent; the percussion, posteriorly, on the right side, is dull as high as the top of the lower third of the scapular region; anteriorly, the sound is on the same side tympanitic, up to about the centre of the mammary region, elsewhere normal; the respiration is vesicular, except over the dull portion, where a fine crepitating râle is heard, followed by bronchial expiration; the pulse is full and accelerated; skin hot and dry; tongue moist. (*Phosphor. the* 3*d dilution, four times daily*) 30th. The dulness on percussion extends posteriorly as high as the centre of the scapular region; laterally the infralateral and lower half of the lateral region are dull; anteriorly, the dulness extends nearly as high as the centre of the mammary space above; for the distance of rather more than one intercostal space the sound is tympanitic, gradually fading into the normal; elsewhere it is normal; the respiration over the dull portion posteriorly is bronchial; anteriorly and elsewhere vesicular; in the lateral region the one passes into the other by means of an intervening undecided respiration. July 1st. Posteriorly, from the centre of the scapular region to the top of it, the sound is to day tympanitic, otherwise as yesterday; the bronchial respiration extends now as high as the top of the scapular region; anteriorly and inferiorly over the dull portion there is no respiration audible; the expectoration is more copious, looking like a dark brown jelly; tongue moist; pulse quiet, and skin more cool. 2d. Physical signs as before, with the addition of a friction-sound at the lower border of the right scapular region; laterally it is inaudible, but anteriorly it is again met with about the centre of the mammary space, where the respiration on auscultating from above begins to vanish; the expectoration is still rusty and blood-streaked. 3d. Expectoration copious, more catarrhal, being simple mucous, with a few blood-streaks; posteriorly a mucous râle, by which the friction-sound is marked, anteriorly it is still audible; otherwise condition the same. 4th. Percussion-sound is posteriorly a little clearer; on auscultation there, a cooing sonorous râle is first heard, followed by a fine crepitating râle; anteriorly the friction-sound has disappeared; expectoration still copious, mucous. 5th. Percussion-sound posteriorly good, anteriorly beginning to clear; posteriorly the respiration is vesicular, with a few crepitating râles. He continued to improve till the 9th, when the vesicular respiration anteriorly began to be heard; the percussion-sound being much improved, but not entirely normal; a little crepitation is still to be heard posteriorly; cough and expectoration slight. 10th. The vesicular respiration is every where restored. 11th. The percussion is to day every where normal; the sole remnant of disease being a few crepitating râles still audible on the right, posteriorly and inferiorly. On the 12th he was discharged.

Case XIV.—E. B., a delicate-looking woman, aged 20, of a clear complexion and nervous temperament, admitted on Sunday, June 21st. On the Monday following, at visit, she stated that she had been unwell for about four days, with feverishness and pain in the chest. She had for some weeks latterly been troubled with a cough, which had within a short time almost entirely ceased, but when it did come, pained her more than ever. The expectoration is rusty and scanty; skin natural, and pulse but slightly accelerated; percussion, anteriorly, dull over right subclavian space, where even the slightest pressure pains her, elsewhere normal; posteriorly, on the right side superiorly dull as far down as the centre of the scapular space, elsewhere normal; respiration is anteriorly vesicular; posteriorly, over the dull space bronchial; beneath it on the right side are numerous mucous râles, on the left vesicular. (*Phosphor. 3d dilution, four times daily.*) 23d. Percussion as yesterday; to-day, over the dull superior portion of the right scapular region a fine crepitation is heard on inspiration, followed by bronchial expiration; the mucous râle is to-day audible anteriorly as well as posteriorly; the expectoration is still rusty, adherent, and scanty. 24th. State the same. 25th. Pain much relieved; percussion unchanged; posteriorly, over dull portion crepitant râle followed by bronchial expiration; anteriorly, superiorly over dull portion consonating mucous râle; inferiorly anteriorly, and posteriorly on the right, mucous râle; expectoration more mucous, still scanty. 26th. Percussion anteriorly as before; posteriorly the dulness extends over rather more than two thirds of the right scapular and interscapular spaces, including also the acromial; on auscultation a consonating mucous râle is heard posteriorly over dull space; inferiorly on the right mucous râle, which is also heard anteriorly, and occasionally over the left side; expectoration more copious, frothy, mucous, mixed with rusty and yellow tougher pieces; tongue red, but moist; pulse more accelerated, rather feeble; skin hot and dry. 27th. The dulness and consonant râle extend to-day over entire right scapular and interscapular spaces; anteriorly the percussion over the superior portion of mammary space is slightly tympanitic, otherwise as before; the auscultation on the right as yesterday; on the left side the quantity of mucous râle is greatly increased, and the thrill caused by it quite perceptible to the touch; tongue posteriorly coated with a red point, inclined to be dry; pulse small and accelerated; expectoration as yesterday, but not so copious; troubled with disagreeable dreams and startings from sleep by night. 28th. Physical signs much the same; expectoration more copious, thin, frothy, mucous; pulse not so fast, and scarcely so thready in character; the skin is cooler and moist; the tongue moist. 29th. Percussion as before; over the lower half of the right scapular and interscapular region a crepitating râle is audible; superiorly a consonating, and inferiorly a non-consonating mucous râle; a quantity of mucous râle audible anteriorly and over the entire left side; the expectoration copious and mucous. 30th. Still improving; skin cool. July 1st. The dullness is now clearing off; posteriorly, on the right side mucous râle without consonance; anteriorly and over the entire left side nothing audible but vesicular respiration; expectoration still copious. 2d. As yesterday, with exception of a fine friction-sound to be heard about the centre of the right scapular space. She continued improving in every respect till the 5th, when the dullness anteriorly was quite gone, posteriorly but slightly perceptible; a fine crepitating râle is heard over the top of the right scapular space; the friction-sound, is gone, having only been audible for two days; the respiration is everywhere vesicular. 7th. The chest sounds everywhere normally on percussion; the respiration is everywhere vesicular, accompanied over the superior portion of the right dorsal region by a few mucous râles, which, by the 9th, had, along with the cough and expectoration, entirely disappeared. The medicine was then stopped. She was allowed to sit up for half an hour daily, and continued gaining rapidly in strength till the 12th, when she was discharged.

Case XV.—A stout-looking young man, of about 25, admitted on Friday, July 10th. At visit, the same day, stated that two days ago, without known cause, he had been seized with pain in the right side, cough and feverishness; he has always previously been healthy, and never subject to cough; he has, he says, within the last day or two expectorated a small quantity of blood; the expectoration is to-day scanty, frothy, mucous, with a streak or two of blood; the percussion anteriorly normal, posteriorly on the right side, dull over the acromial and upper half of the scapular regions; the axillary and upper portion of lateral region on same side are also dull to about their vertical centres; on the left side normal respiration; posteriorly over entire right side, and laterally over dull portion fine crepitating râle; elsewhere vesicular; pulse not much accelerated. (*Phosphor. 3d dilution, four times daily.*) 11th. Right side posteriorly dull over entire surface; otherwise percussion as yesterday, inferiorly and posteriorly on right side; a fine subcrepitant consonating râle over the superior portion of scapular and acromial regions on same side; sonorous râle with occasional bronchial respiration, which is likewise heard over the right axillary and superior portion of lateral regions; left side normal; sputa rusty and scanty. 12th. Percussion posteriorly, as before, laterally clearing off; posteriorly over dull side, mucous consonating râle; crepitation laterally; sputa rusty; pulse quieter; bathed in a copious perspiration. 13th. Skin cool, and still moist, but not perspiring so much as yesterday; physical signs unchanged; sputa mucous. 14th. Percussion-sound clear anteriorly and laterally, posteriorly clearing; sputa mucous and scanty; respiration everywhere vesicular, with an occasional râle where it was formerly dull. 15th. Percussion-sound everywhere normal; an occasional crepitating râle to be heard over the right side posteriorly; respiration everywhere vesicular. 16th. The crepitating râle is to-day only audible on very full inspiration; the cough and expectoration almost entirely gone. Discharged on the 17th.

Case XVI.—J. H., a stout boy, of 10, admitted on the afternoon of Monday, July 13th. Stated next day, at visit, that he had been, previous to admission, only one day unwell. The sputa are few and rusty; cough not very troublesome; pulse full and accelerated; tongue coated, but moist; percussion posteriorly on the left side dull, right normal; over the left lateral and axillary regions tympanitic; anteriorly, on the left, slightly dull, elsewhere normal; respiration, posteriorly on the left, bronchial, elsewhere vesicular. (*Phosphor. 3d dilution, every third hour.*) 15th. His condition was unchanged. 16th. Percussion is to-day dull over the right axillary and lateral regions, otherwise unchanged; over this dull portion bronchial respiration is heard; the auscultation is otherwise unchanged; the pulse is quieter. 17th. The expectoration is to-day more catarrhal; on the left posteriorly the bronchial respiration is not so decided. 18th. Percussion on the left posteriorly still dull; laterally and anteriorly tympanitic; respiration posteriorly on the left is vesicular, preceded by a sonorous râle, elsewhere on the left laterally; rough, vesicular, puerile on the right, and anteriorly on the left normal; expectoration slight. By the 21st the vesicular respiration was audible over the entire chest, and but a slightly perceptible dullness remained on percussing the left side posteriorly, which by the 23d had, along with the cough and expectoration, entirely disappeared. On the 25th his medicine was discontinued, and on the 1st of August he was discharged.

Case XVII.—J. L., a stout boy of 16, admitted July 24th. Stated that he had been ill one day, with pain in the chest and slight cough; the expectoration is very scanty, yellow, and rusty, with a few streaks of blood, adherent; percussion anteriorly normal, posteriorly normal, except the inferior portion of right side, beneath the centre of the scapular space, where it is dull; and over this dull portion is heard on inspiration a subcrepitant râle, followed by bronchial expiration; elsewhere the respiration is normal; the pulse remains quiet; tongue moist, and skin natural. (*Phosphor. 3d dilution, every third hour.*) 25th. The râles are to-day more numerous, almost masking the bronchial expiration; the expectoration copious, of a brownish dirty-looking jelly. 27th. There is to-day a mucous râle over the left side also; the physical signs otherwise unchanged; the pulse has become somewhat accelerated. 28th. Anteriorly the percussion is dull on the right side, beneath and over the lower third of mammary region, also over the right infralateral and lower portion of the latter; a consonating râle is audible over dull space anteriorly and posteriorly; elsewhere vesicular respiration and mucous râle to be heard; skin hot; expectoration as before. 29th. Percussion-sound is anteriorly dull over entire mammary region, superiorly over right subclavicular, somewhat tympanitic; the respiration as formerly; the expectoration scarcely so copious; tongue red, inclined to be dry. 30th. The percussion improved to-day posteriorly; the râle thin, no longer consonates; the pulse is quieter; the skin moist and cool; tongue clean. 31st. Physical signs unchanged; the expectoration is scanty, colourless mucous; pulse quiet; cough not so troublesome. August 1st. Percussion posteriorly clear, anteriorly improved; respiration rough vesicular, puerile, over what was the dull posterior portion, and anterior also; expectoration mucous, still streaked with blood. 3d. The percussion is to-day still slightly dull anteriorly; vesicular respiration; cough almost gone, and expectoration scanty. 4th. Discharged.

To the foregoing must be added two other cases, of which I have not full notes, viz.:

Case XVIII.—A. S., a male, aged 21, admitted May 2d. (*Phosphor. 4th dilution, four times daily.*) Discharged cured after 18 days' treatment.

Case XIX.—F. O., a male, aged 23, admitted 9th May. (*Phosphor. 3d dilution, four times daily.*) Discharged cured after 12 days' treatment.

These cases of Pneumonia give an average age of 24, an average treatment of 12·6 days, and a mortality of 15 per cent., 3 out of 19 having died.

Skoda's cases of Pneumonia, during the same time, amount to 45, his deaths to 3, giving an average of 6·6 per cent.

During this period no bloodletting of any kind was used in Dr. Skoda's wards, his treatment being, extract. graminis ℈j, or nitri puri, or sublimat. ¼ gr., by way of attempting to reduce the plasticity of the blood. He also gave occasionally pulv. Dover, gr. vj, in the course of the day. I regret that, from the patients tiring of one medicine, in no case during these three months did I see a case treated solely with extract. graminis; but hundreds could be extracted from his books treated solely with nitri puri gr. v, ad gr. xx. This seems to be his favorite remedy, though at present he is trying the new chemical theories, and gives the patients sublimate. His average mortality for the last three years, during which no bloodletting—or most rarely—has been performed, and not one leech or cupping-glass applied, is 13·7 per cent, and the recovery of the patients has always been very speedy. He told me that, in the year 1840, he treated 64 females, affected with pneumonia, with large bleedings and large doses of tartar emetic, and only lost one; yet the deaths amongst the males the same year made the total deaths amount to 1 in 8. He considers that this is about the general proportion under all treatments. The great advantage of not bleeding, he considers to be the speedy recovery. The average age was 25·3.

OTHER DISEASES.

During the above period there were eleven cases of TUBERCULOSIS admitted. They were treated chiefly with *sulphur* 4. Two died, and two were left in the house; the others were discharged, some of them a little relieved.

Five cases of CATARRH and two of BRONCHITIS were also admitted; one of the latter following measles. They were treated chiefly with *sulphur, 4th dilution*, and, where the fever ran high, with *aconite, 3d or 4th dilution.*

Two cases of GENERAL SPASMS (at least complained of), were treated—the one with *chamomilla, 6th dilution, twice daily*, the other with *ignatia, 6th dilution, once daily.* They both recovered.

One case of CHOREA was treated with *ignatia, 3d dilution, three times daily*, and was discharged relieved.

Three cases of enlarged SCROFULOUS GLANDS were received and discharged, two in *statu quo*, one after suppuration.

One ASTHMATIC AFFECTION, without apparent cause, except hysteria, in a young girl, aged 18, was treated with *ammon. carb., 3d dilution, four times daily*, and was cured.

A case of GOITRE was treated with *iodine, 2d dilution, four times daily*, and was discharged in five days; the breathing somewhat relieved.

One case of VARIOLA MODIFICATA ran its course regularly without any medicine at all.

One case of ICTERUS, of three weeks' standing, was discharged cured after twenty days; treatment with china, 2d dilution, three times daily.

Six cases of ANGINA TONSILLARIS, slight; treated with belladonna, 3d, 4th, and 6th dilutions, three times daily, and hydrargyrum, 2d and 3d dilutions, four times daily. Average treatment not three days.

One patient with *Catarrhal Ophthalmia* got on her entrance *sulphur, the 3d trituration, four times daily*, but the following day was not to be seen, so I suppose had been quietly discharged.

One case of *old age*, dead.

One case of *General Dropsy*, discharged after five days' treatment with *lactuca, 3d dilution, four times daily.* Improved slightly.

One case of *Exudation in the Peritoneum*, in a young boy, aged 12. The effusion was absorbed during the employment of *bryonia, the 3d dilution, four times daily:* it was followed by an inflammation and suppuration of the tunica reflexa, and the case was left under treatment.

Two cases of *Effusion into the Pleura* were discharged unimproved.

There were in all seventy-one cases in which the patients got no medicine, including several surgical cases, as ulcers, injuries, &c., and one case of burn, which was treated with a turpentine lotion. The other cases were *Chlorosis*, *Headaches*, slight *Stomach Complaints: Expectoration of Blood*, in one case caused by a blow, in another, by hypertrophy of the heart.

But I must now conclude my narrative, incomplete and imperfect as it is. I hope you will excuse the hasty manner in which it has been drawn up, from consideration of the circumstances under which I write. I was anxious to send off my letter before leaving Vienna (which I do to-morrow, August 15th), and I thought you would rather have my account, even in a rough form, early, than in a more digested form at a later period. I possess a complete list of all the patients admitted into the hospital during the three months—a statement of the medicines administered—the length of the treatment, and period of residence in the hospital, &c., which I hope to transmit to you.

I think you will see by what I have stated, that the strength of the homœopathists lies not in the greater rationality or practical superiority of their treatment, but is founded on the weakness of allopathy; that they not only do not help their patients, but—if they are strict homœopaths—are for ever shut out from helping them;—that in their treatment of acute diseases—simpler, at least, if not better than that of their opponents—their success depends entirely on the hitherto unrecognized powers of Nature—all the magic influence of their infinitesimal doses of phosphorus, &c., being emulated if not excelled by the heroic virtues of Extractum Graminis.

I remain, my dear sir, yours faithfully,
George W. Balfour.

REMARKS BY THE EDITOR.

The preceding Report may be truly said to speak for itself, yet it may not be inexpedient to advert to a few of the more important particulars contained in it, especially those which bear most pointedly on the main questions to which this part of our Journal is for the present devoted.

1. In the first place, it seems clearly made out, from Dr. Balfour's account—confirmed and corroborated as it is in this point by Dr. Mühry's letter—that what is now called Homœopathy, is far from being, either in principle or practice, always or necessarily the system first promulgated by Hahnemann under this name. We think it evident, from Dr. Balfour's cases, that even Hahnemann's avowed follower, Fleischmann, sets little store by the fundamental principle, *similia similibus*, in the administration of his remedies.

2. Secondly, the facile and uniform attainment of the same triumphant results under all these varieties of the homœopathic system, points to the operation of one and the same curative agent in all—namely, our old friend, the *Vis Medicatrix.*

3. The great and manifold divisions already existing among the homœopathists, seem to lead irresistibly to the conclusion that the beginning of the end of homœopathy is already come. If we have the very best authority for believing that a house divided against itself cannot stand, we can hardly doubt that the house of Hahnemann is now tottering to its fall.

4. It is sufficiently evident from many passages in the Report, that whatever might have been the comparative general result of adopting the ordinary (allopathic) treatment in the cases related, its adoption would have been decidedly advantageous in particular instances, by affording aid to nature in relieving distressing symptoms, which were not relieved, nor attempted to be relieved, by the homœopathic treatment. Whether homœopathy be true or false, there can be no doubt that Dr. Fleischmann's treatment was often bad.

5. But whatever may be thought of homœopathy as a doctrine, or as a general system of practice, it will not be doubted, after this Report, that the

more common acute diseases may and can, during a certain period of time at least, run their course favorably enough under its administration. The general results of the treatment during the three months reported of, were we think, such as would have satisfied many practitioners of our ordinary medicine.

6. Neither will it any longer be denied that PNEUMONIA, even in a severe form, may, under homœopathic treatment, pass through all its stages to perfect recovery.

7. The great and important practical question is—Whether or not the homœopathic remedies administered in these cases contributed in any degree—or if in some degree, in what degree—towards the cure of the diseases, particularly the cases of pneumonia? This is a question which will be answered differently by different persons. No doubt, Dr. Fleischmann and homœopathists generally will regard these cases not only as highly favorable to the claims of homœopathy, but as unquestionable proofs of its great remedial powers We, on the contrary, in common with our reporter, see no other powers operating in these cases but the natural powers of the living system, called into action under very favorable circumstances. The general aspect of the whole cases, favorable and unfavorable alike, and the minute details of each case, convey to our mind the most perfect conviction that, throughout, Nature, not art, was the worker; and it would seem easy to convey to others the grounds of this conviction, by a minute analysis of the individual cases through their whole progress, whether the patients were getting better or getting worse, whether taking or omitting the homœopathic medicaments.

8. The same doubts as to the efficacy of the medicines employed may exist in regard to the cases of Professor Skoda. But if the advocates of homœopathy insist on the value of the evidence in favour of the deillionths of phosphorus, aconite, or bryonia, they surely have no right to reject the evidence of a precisely similar kind, in favour of Dr. Skoda's grain doses of the extractum graminis or nitre. We may perhaps be forgiven even by the most zealous partisans of ordinary medicine, particularly the abettors of the heroic system, for setting down both systems as equally effective in the case in question, that is, as having no effect at all.

9. The results of the treatment of the cases of pneumonia, both by Fleischmann and Skoda, seem to prove that bloodletting is not so essential and indispensable a remedy, even in severe cases of pneumonia, as has usually been supposed; and that its omission, in a certain proportion of cases at least, does not even retard the period of convalescence or the completion of the cure.

10. The materials supplied in Dr. Balfour's Report, like those formerly supplied in Dr. Fleischmann's, do not in any degree authorize the general conclusion that homœopathic treatment is as good as that of ordinary medicine, much less that the latter like the former is valueless, Nature being all-sufficient in the cure of diseases; but both go powerfully to corroborate the following, among other important inferences formerly deduced by us from a review of the whole question, viz.—1. That Nature is more powerful in curing diseases, and has practically a much greater share in the ordinary cure of diseases, than is commonly believed. 2. That the prevailing doctrines respecting the actions and powers of many particular medicaments and other so-called remedial agents, require reinvestigation, with a view to ascertain the truth. 3. That while this investigation is in progress, we should be cautious, even timid, in the use of medicaments and other medical means capable of exerting a powerful influence on the animal system, and therefore capable (possibly) of doing evil as well as good. 4. That in the present state of our knowledge, the Hygienic—Eclectic—Hippocratic—Natural system of treating diseases, is the only one that can be justified or safely followed.

BOOKS RECEIVED FOR REVIEW.

1. The Sanative Influence of Climate. By Sir James Clark, Bart. Fourth edition. London, 1846. 8vo, pp. 412. 10s. 6d.

2. Practical Surgery By Robert Liston. Fourth edition, London, 1846. 8vo, pp. 582 22s.

3. Observations on the Principle of Vital Affinity, as illustrated by Recent Discoveries in Chemistry. By W. P. Alison, M.D., F.R.S.E. (From the Transactions of the Royal Society of Edinburgh.) Edinburgh, 1846. 8vo, pp. 23.

4. The Open Surgery Question considered. By W. J. Preston. London, 1846. 8vo, pp. 25.

5. Meade's Manual for Students preparing for Examination at Apothecaries' Hall. Second edition. London, 1846. 8vo, pp. 586. 10s. 6d.

6. The Economy of the Animal Kingdom, considered Anatomically, Physically, and Physiologically. By Emanuel Swedenborg. Translated from the Latin by the Rev. A. Clesseld, M.D. Two vols. 8vo, pp. 574-426.

7. Verbrennung und Athmen, chemische Thätigkeit und organisches Leben. Von Fr. Nasse. Bonn, 1846. 8vo, pp. 154.

8. Introductory Lecture to a Course of Military Surgery. By Sir George Ballingall, M.D. Edinburgh, 1846. 8vo, pp 23.

9. Fragments of Medical Science and Art. An Address. By H. J. Bigelow, M.D. Boston, 1846. 8vo, pp. 54.

10. A Series of Essays on Inflammation and its Varieties. Essay I. The Natural History of the Disease. By Henry Clutterbuck, M D. London, 1846. 8vo, pp. 67.

11. Three Reports of the Joint Deputation of the Society of Apothecaries and the National Association. London, 1846. 8vo, pp. 46. 1s.

12. Analysis of the Evidence in Favour of the Constant Supply System. By J. Wicksteed, Esq. London, 1846. 8vo, pp. 46.

13. Inaugural Dissertation on Yellow Fever. By Hugh Bone, M.D. Edin. 1846. 8vo, pp. 85.

14. Historical and Critical Remarks on the Operation for the Cure of Cataract. By A. Watson, M.D. Edinburgh, 1845. 8vo, pp. 35.

15. On the Treatment of Strictures of the Urethra. By James Briggs, Surgeon. London, 1845. 8vo, pp. 62. 2s. 6d.

16. On the Health of Towns, as influenced by Defective Cleansing and Drainage. By W. A. Guy, M.D. London. 1846. 8vo, pp. 48. 1s.

17. On the Incubation of Insanity. By Forbes Winslow, M.D London, 1846. 8vo, pp. 23.

18. On the Injurious Effects of Mineral Poisons in the Practice of Medicine. By H. Prater, M.D. London, 1846. 8vo, pp. 96.

19. Lectures on the Urine, and on the Pathology, Diagnosis, and Treatment of Urinary Diseases. By J. Aldridge, M.D. Dublin, 1846. 8vo, pp. 80. 2s. 6d.

20. Kissingen. By N. A. Travis, M.D. Francfort. 12mo, pp. 32. 1s.

21. Homœopathy, Allopathy, and Young Physic. By John Forbes, M.D. Philadelphia, 1846. 8vo, pp. 120.

22. The Use of the Body in relation to the Mind By G. Moore, M.D. London, 1846. 8vo, pp 431. 9s.

23. The Surgical, Mechanical, and Medical Treatment of the Teeth. By J. Robinson. London. 1846. 8vo, pp. 320. 10s.

24. Medical Report of the House of Recovery and Fever Hospital, Boyle street, Dublin. By G. A. Kennedy, M.D Dublin, 1846. 8vo, pp. 52.

25. Experimental Researches on the Food of Animals, with Remarks on the Food of Man. By R. D. Thomson, M.D. London, 1846. 8vo, pp. 195.

26. Elements of Physics. By C. F. Peschel. Translated from the German by E. West. 3 vols. London, 1846. 8vo.

27 Practical Observations on Mineral Waters and Baths. By E. Lee. London, 1846. 8vo, pp. 134. 4s 6d.

28. Dr. Hooper's Physician's Vade Mecum. By Dr Guy. New edition. 1846. 8vo, pp. 524. 10s. 6d.

29. A Medical Topography of Tunbridge Wells By R. H. Powell, M.D. London, 1846. 8vo, pp. 174. 3s. 6d.

30. Clinical Collections and Observations in Surgery. By W. P. Ormerod, Surgeon. Lond. 1846. 8vo, pp. 312.

31. Fever Physiologically considered. By D. M. Reed. London, 1846. 8vo, pp. 262. 10s

32. The Antidotal Treatment of Cholera. By R. Parkin, M.D. London, 1846. 8vo, pp. 48. 2s. 6d.

33. The Microscopical Anatomy of the Human Body. By A. H. Hassall, F.L.S. 8vo. 1846. Parts I and II. 2s. 6d. each.

34. A Practical Treatise on the Diseases of Children. By J. M. Coley, M.D. London, 1846. 8vo, pp. 467. 14s.

35. Notes on the Epidemic Cholera of India. By A. H. Kennedy. London, 1846. 8vo. 10s. 6d.

36. Handbuch der Pathologie und Therapie. Von Dr. C. A. Wunderlich, Professor zu Tübingen. Stuttgart, 1846.

37. The Harveian Oration, delivered before the Royal College of Physicians. By John Elliotson, M.D., F.R.S. London, 1846. 8vo, pp. 70. 2s.

38. The Blood, Anatomically, Physiologically, and Pathologically considered. The Warneford Prize Essay for 1845. By C. E. Joseph, M.R.C.S. London, 1846. 8vo, pp. 126

39. Observations on the Edinburgh Pharmacopœia. By Richard Phillips, F.R.S., &c. Lond. 1846. 8vo, pp. 56

40. The Mineral Waters of Kreutznach. By J. E. P. Prieger, M.D. Translated by O. Prieger, M.D. London, 1846. 8vo, pp. 92. 3s. 6d.

INDEX TO VOL. XXII.

OF THE

BRITISH AND FOREIGN MEDICAL REVIEW.

PAGE

Alison, Dr. S. on alterations of the heart 234
Amputation of limbs, statistics of . 52
Amputation at the hip-joint, treatise on 111
Anatomy, pathological, Dr. Vogel's 323
Antagonism of phthisis and fever 213
Aneurism, Mr. Liston's treatment of 373
Aphorisms in medicine, Dr. Pauli's 53
Apoplexy, its connexion with heart-disease 408-20
  influence of age on 421
  treatment of 428
  Dr. Huss's report on 461
Arachnitis, history and treatment of 296
Army, on the field-carriage of 225
Arteries, Mr. Quain's great work on 556
Arteries, Professor Porta's work on 89
  experiments on the ligature of 91
  Porta, on the torsion of 102
Arteries and nerves, plates of 231
Association, Provincial, transactions of 319
Asthma, grinders', Dr. Favell on 320
Atlas of plates, Dr. Mouatt's 227
Auscultation and percussion, Gunzburg on 232
Austria, history of foundlings in 338

Balfour, Dr. his report on homœopathy 567
Bartlett, Dr. on the progress and prospects of medicine 235
Baynard, Dr. his anticipation of Liebig 433
Bladder, cancer of 305
  prolapsed, case of 320
Blood and nerves, Dr. Heidler on 219
Blood and urine, analysis of 223
Blood, pathological relations of 326

PAGE

Blood-vessels, Mr. Liston on injuries of 372
Bones, hereditary brittleness of 49
  Mr. Liston on the injuries of 369
Bonnet, M. on diseases of the joints 62
Books received for review 292-594
Brain, on the physiology of 488
  its special functions 516
Boudin, Mr. on the causes of phthisis and fever 211
Bowditch, Dr. his young stethoscopist 209
Brain, inflammation of the membranes of 385
  on the physiology of 488
  comparative anatomy of 494
  and heart, relation of the diseases of 408
Breast, Sir B. Brodie on tumours of 167
  on the diseases of 156
  female, Mr. Tuson on 173
Brodie, Sir B. his lectures on surgery 160
Bronchocele, operation for 50
Burrows, Dr. on disorders of the cerebral circulation 408

Cancer, M. Lebert on 32
  Dr. Walshe's treatise on 293
Carbonic acid, on the quantity exhaled 189
Carotids, effects of ligature of 417
Carpenter, Dr. on the structure of the brain 501
Cerebellum, its functions 529
  comparative anatomy of 535
Cerebral circulation, Dr. Burrows on 408
Cerebrum, not the seat of sensation 504
  its functions 511
Cerebro-spinal affections and heart-disease 425

PAGE

Cholera, its treatment by homœopathy 580
Children, on the diseases of . . 548
China, Dr. Wilson's medical notes on 179
Circulation, cerebral, Dr. Burrows on 408
Clark, Sir James, his influence of climate 228
Climate, Sir James Clark on the influence of 228
Cold-water cure, treatises on . 428
Coley, Dr. on diseases of children 548
Colic, its treatment by homœopathy 580
Collections, medical, Mr. Maclure's 224
College of physicians cannot make *Doctors* 549
Coma, causes of 418
Combe, Dr. A. on phrenology . 230
Correspondence with the editor on "Young Physic" 243
Cox, Mr. on amputation at the hip-joint 111
Crosse, Mr. his case of prolapsed bladder 320
Currie, Dr. on cold affusion . . . 434

Diarrhœa, its treatment by homœopathy 579
Doctor of Medicine, on the title of 549
Dropsy, pathological anatomy of . 325
Drowning, Sir B. Brodie on . . 164
Dysentery and fever in China . 185
    its treatment by homœopathy . . . 579

Encephalon, cancer of 309
Escharotics, Mr. Liston on the use of 368
Esdaile, Dr. on mesmerism in India 475
Evans, Dr. his lectures on phthisis 85

Farcy, Mr. Percival on . . . 47
Fergusson, Dr. his notes and recollections 551
Fever, its treatment by cold water 446
    and dysentery in China . 182
    history and mystery of its treatment 552
    gastric, its treatment by homœopathy 576
    intermittent, its treatment by homœopathy . . ib.
Fleischmann, his homœopathic treatment 567
Floyer, Sir John, his work on cold water 432
Foundlings, history of, in Austria 338
Fractures, ununited, Sir B. Brodie on 166

Gastrodynia and gastritis, diagnosis of 297
Genital organs, cancer of . . 306

PAGE

Geological relations of phthisis and fever 211
Gall, his doctrine of the cerebellum 534
Ganglia, cephalic, their functions . 495
Germany, state of homœopathy in 565
Gilchrist, Dr. on the natural treatment of wounds . . . 238
Gintrac, M. intra-thoracic tumours 216
Glanders and farcy, Percival on . 35
Glanders, symptoms of . . . 37
    varieties of . . . 39
    causes of 40
    pathology of . . . 45
Glasgow, the vital statistics of . 215
Grinders' asthma, Dr. Favell on . 320
Gums, cancer of 294
Günzburg on percussion . . 232

Hannover, Dr. on the exhalation of carbonic acid 189
Hassall, Mr. his microscopic anatomy 552
Headache in diseases of the heart 425
Heart and brain, on the relation of diseases of 408
Heidler, Dr. on blood and nerves . 219
Hemorrhagia cerebri, diagnosis of 387
Hernia, and the operation for . 51
    Mr. Teale's treatise on . 193
    on dividing the stricture in 377
Hip-joint, treatise on the amputation at 111
Homœopathic treatment, editor's remarks on 592
Homœopathy, its history in Germany . 565, 567
    report on . . 567
Horaczek, Dr. on icterus . . 311
Hospital, homœopathic, at Vienna 570
Huss, Dr. his report on diseases . 459
Hydrocephalus acutus, case of . 403
Hydropathy, treatises on . . 428
    its physiological and therapeutic effects . . . 441

Icterus, or bilious dyscrasy, treatise on 311
Indians, their treatment by water . 435
Inflammation, Dr. Lebert on . 19
    of the membranes of the brain . . 385
Injections, use of in hydrarthrosis, &c. 71
Insanity, Dr. Jacobi on the form of 1
    cases of by Dr. Jacobi 3, 5, 7, 9
    Dr. Jacobi on the phenomena of . . . 12, 16
    on the treatment of . . 55
    use of narcotics in . . 55
    Dr. Steward's classification of 59

PAGE

- Insanity, moral treatment of . 60
  - Dr. Thurnam's statistics of 349
  - relative liability to . . 360
- Intestines, cancer of . . . 299
- Irritability, nervous, from heart disease 425

- Jacobi, Dr., on the forms of insanity 1
  - account of his writings 2
- Joints, treatise on the diseases of . 62
  - causes of diseases of . . 63
  - treatment of diseases of . 65
  - Mr. Liston on the diseases of 370

- Kidney, cancer of 303

- Lanzani, his work on cold water . 431
- Lebert, Dr., his pathological physiology 18
- Leeson, Mr., on the heart and lungs, &c. 221
- Liebig travestied 221
- Limb, on the shortening and lengthening of in hip-cases . . . 78
- Lindley, Mr., his vegetable kingdom 554
- Lips, cancer of 293
- Liston, Mr., his practical surgery . 366
- Lithotrity, Mr. Liston on . . 376
- Liver, cancer of 300
- Login, Dr., on army-field-carriage . 225
- Lungs, cancer of 301

- Maclure, Mr., his collectanea medica 226
- Mammæ, on the diseases of . . 156
  - hypertrophy of . . ib.
- Mania without illusion, case of . 3, 7
  - with delirium, case of . . 5, 9
- Medicine, its progress and prospects 235
- Melzer, Dr., his history of foundlings 338
- Mericourt, M., on diseases of the breast 156
- Mesmerism in India . . . 475
- Microscopic anatomy, Hassall on . 552
- Milk abscess 157
- Morbid growths, Mr. Liston on exploration of 374
- Mouatt, Dr., his Hindustani anatomy 227
- Mühry, Dr., on homœopathy in Germany 565
- Mütter, Dr., his edition of Liston's lectures 379

- Narcotics, their use in insanity . 55
- Nature, on excessive trust in . . 557
- Neisser on inflammation of the membranes of the brain . . 355

PAGE

- Nerves and blood, Dr. Heidler on . 219
  - arteries, plates of . . 231
- Newport, Mr., on the structure of the brain 499
- Noble, Mr., on the physiology of the brain 488
- Nose, Dr. Mütter's formation of a . 381

- Œsophagus, cancer of . . . 295
- Operations, employment of mesmerism 475
- Osteoma frontalis, case of . . 462
- Ovaries, cancer of 307

- Paget, Mr., his report on anatomy and physiology 261
- Pathology, brute, its importance . 35
- Pauli, Dr. his observations in surgery 49
- Percival, Mr., on glanders and farcy 35
- Peschel, M., his elements of physics 553
- Phillips, Mr., his treatise on scrofula 124
- Phrenology, its nature and uses . 236
  - criticism on its doctrine . . 488–517
- Physics, Peschel's elements of . 553
- Physiology of the brain, Mr. Noble on 488
- Phthisis, Dr. Evans's lectures on . 85
  - and scrofula, are they identical 132
- Physician, on the deserts and duties of 115
  - his *saper-fare* or *savoir-faire* 117
- Physiology, pathological, Dr. Lebert on 18
- Pneumonia, Dr. Huss on . . 468
  - its treatment by homœopathy . . . 581
  - view of cases of . . ib.
- Poisons, mineral, Dr. Prater on . 545
- Polycholia 312
- Porta, Professor, on ligature of arteries 89
- Practitioners, portraits of various leading 121
- Prater, Dr. on mineral poisons . 545
- Predisposition to scrofula, on the . 144
- Pressure in the treatment of aneurism 377
  - vascular, within the cranium 413
- Priessnitz, his system of medicine . 436
- Products, morbid, pathology of . 329
- Pus, Dr. Lebert on 21
  - pathology of 330

- Quain, his great work on the arteries 556

- Ratier, M., on syphilis . . . 226
- Rees, Dr., on blood and urine . 223
- Report on anatomy and physiology, by Mr. Paget 261
- Retreat, the York, history of . . 363
- Retzius, Professor, on the structure of the brain 502

PAGE

- Rheumatism, its treatment by homœopathy 577

- Sava, Dr. on the duties of the physician 115
- Scarlatina, its treatment by homœopathy 581
- Skoda, Professor, his treatment of pneumonia 593
- Scrofula, Mr. Phillips's treatise on 124
  - statistics of 125
  - signs of 127
  - and phthisis, are they identical 132
  - prevalence of, in different countries 138
  - on the causes of 145
  - treatment of 150
- Scrofulous matter, its characters and source 130
- Spinal irritation, Dr. Huss on 403
  - treatment of 466
- Stethoscopist, the young 209
- Steward, Dr., on insanity 55
- Stockholm, report of diseases in 459
- Stomach, cancer of 296
- Strangulation, Sir B. Brodie on the effects of 161
- Surgery, Dr. Pauli's researches on 49
  - Sir B. Brodie's lectures on 160
  - practical, Mr. Liston's 366
  - Mr. Liston's lectures on 379
  - application of mesmerism in 475
- Symonds, Dr., on excessive trust in nature 557
- Syphilis, Sir B. Brodie, on mercury in 169
  - M. Ratier on 226

- Taylor, Mr., on the temperature of the earth 229
- Teale, Mr., his treatise on hernia 193

PAGE

- Temperature of the earth and sea 229
- Thurnam, Dr. his statistics of insanity 349
- Tongue, cancer of 294
- Torsion of arteries, Professor Porta on 101
- Transactions of provincial association 319
- Tubercle, anatomy and chemistry of 27
- Tumours, pathological physiology of 29
  - intra-thoracic, treatise on 216
  - pathology and classification of 334
- Tuson, Mr., on the female breast 173
- Typhus fever, Dr. Huss on 269
  - its treatment by homœopathy 573

- Urine, Dr. Rees on the analysis of 223
- Urology, Dr. Leroy-d'Etiolles on 233
- Uterus, polypus and inversion of 51

- Vegetable kingdom, Prof. Lindley's 554
- Vis medicatrix naturæ, requires guiding 557
- Vienna, homœopathic hospital of 567
- Vital statistics of Glasgow 215
- Vogel, Professor, his pathological anatomy 323

- Water-cure, treatises on 428
- Watt, Dr., on the statistics of Glasgow 213
- Walshe, Dr., on cancer 293
- Williams, Dr., on insanity 55
- Wilson, Dr., his medical notes on China 179
- Wounds, on the natural treatment of 238

- "Young Physic," correspondence respecting 240

END OF VOL. XXII.

THE

BRITISH AND FOREIGN

MEDICAL REVIEW:

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

EDITED BY

JOHN FORBES, M.D. F.R.S. F.G.S.

VOL. XXII.

JULY—OCTOBER, 1846.

PHILADELPHIA:
G. B. ZIEBER & CO.
1846.

C. SHERMAN, PRINTER.
19 St. James Street.